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*Thos Earl Stanthorne.*



AGRICULTURAL RESEARCH INSTITUTE  
PUSA





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PHILOSOPHICAL  
TRANSACTIONS,

GIVING SOME

A C C O U N T

O F T H E

Present Undertakings, Studies, *and* Labours,

O F T H E

I N G E N I O U S.

I N M A N Y

Confiderable Parts of the W O R L D.

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VOL. XLVII. For the Years 1751 and 1752.

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L O N D O N:

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M. DCC. LIII.

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## ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from the repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till this present XLVII. volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the royal charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future

## ADVERTISEMENT.

*Transactions* ; which was accordingly done upon the 26 of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them ; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of nature or art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons, thro whose hands they receive them, are to be considered in no other light, than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society ; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices ; which in some instances have been too lightly credited, to the dishonour of the Society.

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- I. *A Letter from Mr. Wm. Smith to Mr. Robert Aulten, concerning a Fire-ball, seen in the Air July 22. 1750. communicated to the Royal Society by the Rev. Wm. Stukeley M. D. F. R. S. and Rector of St. George the Martyr, London.*

Read Jan. 10. 1750. **Y**OUR leaving this place somewhat sooner than I expected, prevented my giving you an account of that beautiful phenomenon, that appeared this last summer at this place, and the neighbouring villages, which I told you of. I therefore give you the trouble hereof, that you may, if you think it worth your time, communicate the same to Dr. Stukeley.

On Sunday the 22 of July last about 20 minutes before 9, as near as I can remember, in the evening, as I came from Werrington, two miles north-west of this place, I saw to the left of me (as did two others then in company with me) and seemingly about the height of the sun when about two hours high, a ball of light, bigger than a star of the first magnitude to our appearance; the colour like that of a rocket, when thrown, and in its full glory. It drew a tail of light, to our view about 3 feet and a half long, which was broadest and brightest next the ball, and grew taper in form, and languid in colour, to its termination. Its course was about north-west to south-west. It moved in a direct line  
A horizontally,

horizontally, and its motion thro' the air was little swifter than the passage of a duck, hawk, or pigeon, in their flight.-

We had the view of it for about three fourths of a minute ; but, being in the road near the north end of Walton, and under the trees, lost sight of it sooner than I desired.

Several people coming from Peterborough, and on the south side of the town coming from Fletton, saw the same, and give the same account of it as I have above done.

I heard it was seen at Bourn, which is north-west 12 miles off us, in the same manner. It must consequently be at a great height from us (tho' it did not seem to be so) by reason people in Borough-Fen, which lies north-east of the place where I was when I saw it, saw the same on the same hand I did, and its form and course in the same manner.

The veracity of this account may be depended on, as many others saw the same, as well as,

Dear Sir,

Peterborough, Dec. 13.

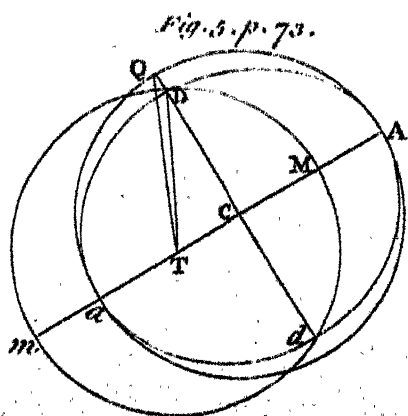
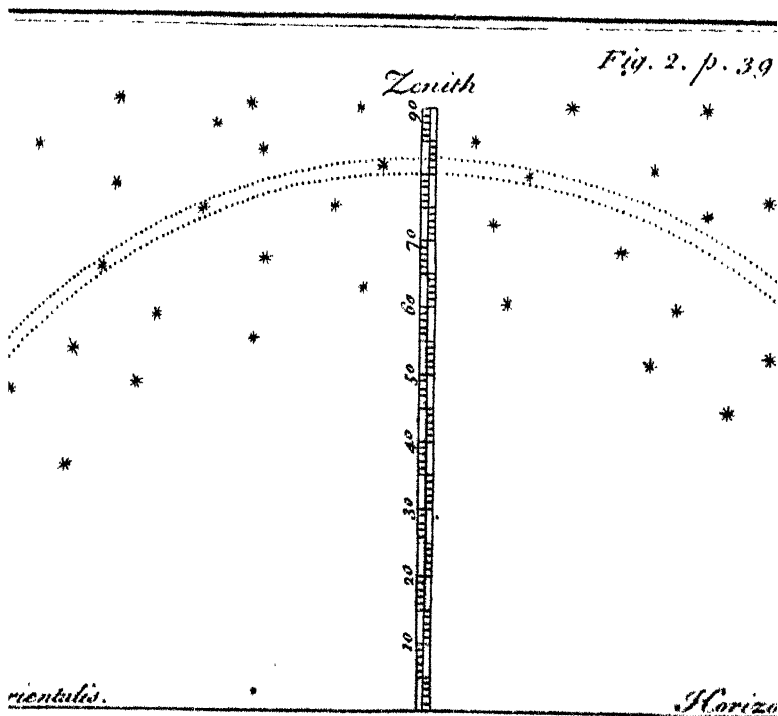
1750.

Your very humble servant,

W. Smith.







*Fig. 1. p. 3.*





II. *An Account of the same Meteor, by Mr. Henry Baker F. R. S. in a Letter to M. Folkes Esq; Pr. R. S.*

S I R,

Read Jan. 10.  
1750.

**A**S I know of no account, that has been yet communicated to the Royal Society, of a fiery meteor, seen in many distant parts of this kingdom in July last, I hope you will excuse the liberty I take of laying before you what I have receiv'd concerning that appearance.

On the 28 of the said month of July, Mr. William Arderon F. R. S. wrote me word, that a meteor was seen at Norwich by thousands of people, on Sunday the 22 of the said month, at 9 o'clock in the evening (true time). He sent me also a drawing thereof, which is exactly copied at the end of this paper. (Plate I. Fig. 1.)

Its direction, he says, was, as near as he could guess, from north to south, moving with great velocity. When due east of him, its altitude was about 30 degrees; at which time the great distinctness of its figure made him imagine it was not above two or three miles from him. The splendor and beauty of its nucleus, particularly the fore part thereof, surpassed, he says, all the fires he ever saw, being of a bright silver colour: its tail was of the colour of a burning coal, tho' something fainter. Its head, or nucleus, appeared to him, under an angle of somewhat more than two degrees, and its tail of about 21 degrees.

He lost sight of it in a cloud, not above 20 degrees above the southern part of the horizon, into the middle of which it enter'd: but a friend of his, being about 4 miles more southward, saw it again, after it came out of this cloud, till it enter'd into another.

The excessive hot weather in the preceding part of the month of July, especially on Wednesday the 11th day thereof, which is supposed to have been the hottest day we have had for many years in England, may perhaps account, in some measure, for the generation of this fiery meteor.

I intirely submit to you, whether it deserves to be taken notice of by the Royal Society, among the extraordinary phænomena of the year 1750; and am, with the greatest respect,

S I R,

Your most obedient humble servant,

Catharine-street, Dec. 12.

1750.

H. Baker.

III. Thermometrical Tables *and* Observations,  
in a Letter to John Pringle M. D. &  
F. R. S. by John Stedman M. D.

S I R,

Read Jan. 10.  
1750.

I ACQUAINTED you some time since of having kept a journal of the weather in the camp, whilst I attended the army in the Netherlands; and that, having given particular attention

tention to the thermometer, I found, that the heat in tents was remarkable for its degree, sudden and great vicissitudes, and almost continual variation from the state of the open air. As a specimen of this, and in compliance with your desire, I have sent you a table of my observations on this subject, during our incampment in Dutch Brabant, in the last year of the war.

It will be proper to observe, that, to keep the thermometers, placed in the open air, from the direct rays of the sun, it was necessary to suspend them so low, that the reflexion of heat from the earth must sometimes have rais'd the Mercury higher, than would have happen'd, had the instruments been remov'd farther from the ground ; and it must also be remember'd, that, for some days of this season, the weather was uncommonly warm.

In keeping this journal, I observ'd,

1. That, in tents, the heat frequently varies 20, 25, and sometimes 30 degrees in twenty-four hours; reckoning by Fahrenheit's scale.

2. That the uneasiness, felt upon great changes of heat and cold, depends more upon the sudden change from the one to the other, than upon the excess of either ; having often seen, in a long course of sultry weather, men sitting unconcernedly in their tents, when the air they breath'd in was rais'd to about 90 degrees ; and the same men in winter standing in the open air with no warmer cloaths, and yet without any complaint, tho' the cold was some degrees below the freezing point. Whence it appears, that, if such a change of air be gradual, the same person can, without any uneasy sensation,  
bear

bear the difference of 60, 62, or 64 degrees of heat.

3. That we are able to endure a greater degree of heat, than what has been hitherto thought enough to kill animals, as will appear from the following example :

A soldier being confin'd to a tent call'd the stand-ard-guard, while the weather was so extremely hot, that the thermometer rose within the tent 103 or 104 degrees \*; on the second day his pulse was quick and full, his mouth foul, and he complain'd of thirst, a nausea, and head-ach. A thermometer being then kept for some time in his arm-pit, rose to 106 degrees. On the third day all the symptoms increased; tho' the thermometer applied to his body, rose no higher than the day before: but, upon my representing the danger from the heat, he was enlarged, and thereupon immediately recover'd.

The heat in this instance was several degrees beyond what the learned professor Boerhaave thought sufficient to coagulate the blood.

4. That a damp air (*cæteris paribus*) gives a sensation of greater heat or cold than a dry air; *viz.* a sensation of greater heat, when the Mercury is about 70 degrees or upwards; and of cold, when about 50 degrees, or below that point.

5. That we are able to endure the open air, when heated to a degree considerably greater than the air of a room, that is heated by a fire: and, since one may stay some hours in a bagnio, where the heat  
is

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\* By Fahrenheit's scale.

is at 100 degrees †, we may conclude, that the open air, heated to that degree, will be suffer'd with less uneasiness, than when it is so confined.

6. That medicines, for whose operation a pretty high degree of heat is necessary, cannot be taken safely, where the heat is very variable, tho' it should not be less than the degree requisite for the working of such medicines. Thus a mercurial salivation may be carried on safely, where the heat is kept from 66 to 72 degrees; but, were the heat suddenly to vary 15 or 20 degrees, the change would be dangerous, tho' the heat was not to fall below 66 degrees.

7. That the body is sometimes differently affected, according to the different constitutions of the air; tho' the air remains the same, so far as we can judge, with regard to heat, humidity, and gravity.

8. That, when the thermometer is high, our bodies are very sensible of a small addition of heat: but it is uncertain, whether this proceeds from the heat being near the greatest degree we can bear; or, that a greater proportion of heat is requisite to raise the thermometer the same number of degrees after it is high, than when it is low. If this be the case, then, in graduating the thermometers, the degrees ought to be marked shorter, proportionally to the height of the mercury; but in what proportion, is not yet discovered. I am, &c.

Edinburgh, Oct. 3.  
1750.

TABLE of HEAT.  
Campaign 1748.

Months. N. S.	Days.	Hours.		Deg. <i>Fahrenh.</i>		Soldiers Tent.	R. Societ. Therm.	Eainb. Therm.
		A. Mer.	P. Mer.	Officers Tent.	Open Air.		Open Air.	
<i>April</i>	10	6		48	42	44	65	10.4
			2	59	56	62	45	13.5
			10	47	40	42	68	10.
	11	5		38	34	34	76	8.5
			1	52	52	58	51	12.6
			11	42	39	39	69	9.8
	12	7		44	44	44	63	10.8
			1	52	50	61	54	12.1
			11	50	47	47	58	11.6
	13	6		40	36	36	73	9.
			1	62	58	67	43	13.9
			11	40	35	35	75	8.8
	14	5		41	34	34	76	8.5
			3	70	67	72	30	15.8
	16	7		57	52	56	51	12.6
			1	70	65	70	33	15.2
			10	52	49	49	56	11.9
	17	7		38	34	34	76	8.5
			2	47	46	52	60	11.2
			11	40	37	37	73	9.3

Months.

Months. N. S.	Days.	Hours.		Deg. Tent.	Fahrenheit.		R. Societ. Therm.	Edinb. Therm.
		A. Mer.	P. Mer.		Open Air.	Soldiers Tent.	Open	Air.
<i>April</i>	19	6		40	40		68	10.
			2	48	42		66	10.4
			10	41	38		71	9.5
	20	7		39	32	33	79	8.2
			1	61	58	67	43	13.9
			11	40	38	37	71	9.5
	21	7		47	40	42	68	10.
		11		48	44	45	62	10.8
			10	40	38	40	71	9.5
	22	8		40	37	37	73	9.2
			2	52	50	59	53	12.1
			11	38	34	34	76	8.6
	23	8		40	40		68	10.
			1	51	48		57	11.7
	24	7		40	37	38	73	9.2
			2	50	47	50	58	11.6
			11	42	37	39	73	9.2
	26	7		46	42		65	10.4
			1	51	49		56	11.9
	27	6		40	36	36	74	9.
			1	40	40	38	68	10.
			7	42	42	38	65	10.4
			11	40	37	37	73	9.2



Months N. S.	Days.	Hours.		Deg. <i>Fahrenh.</i>			<i>Emb.</i> Therm.	
		<i>A. Mer.</i>	<i>P. Mer.</i>	Officers Tent.	Open Air.	Soldiers Tent.	R. Societ Therm.	Open Air.
<i>April</i>	28	7		54	42	44	65	10.4
		11		48	45	48	63	11.
			8	42	38	42	71	9.5
	29	6		42	33		78	8.4
		11		49	42		65	10.4
			5	47	38		71	9.5
	30	6		46	32		79	8.3
		11		52	40		68	10.
			9	50	48		57	11.7
			11	47	44		62	10.8
<i>May</i>	1	7		46	47	47	58	11.5
			1	47	47	47	58	11.5
			10	44	40	42	68	10.
	2	7		49	47	53	58	11.5
		11		60	64	64	34	15.1
			10	48	44	47	62	10.8
	3	8		56	54		49	13.
			1	60	56		46	13.4
			10	48	44		62	10.8
	4	8		66	64		34	15.1
			1	74	78		15	18.1
			10	56	54		49	13.
	5	8		64	64		34	15.1

Months

Months. N. S.	Days.	Hours.		Deg. <i>Fabre.</i> <i>b.</i>			Therm. R. Societ.	Therm. <i>Edinb.</i>
		<i>A. Mer.</i>	<i>P. Mer.</i>	Tent.	Officers Air.	Open Air.		
<i>May</i>	5		1	76	82		9	19.
			11	64	62		37	14.6
	6	8		63	60	63	40	14.2
			2	63	62	63	37	14.6
			11	56	56	56	46	13.4
				60	56	58	46	13.4
	7	8	10	53	50		54	12.1
				64	62		37	14.6
	8	7	1	69	78		15	18.1
				59	57		45	13.7
	9	2		59	56		46	13.4
			8		66	66		32
			2	66	66		32	15.5
			3	64	72	76	23	16.8
			11	52	54		48	13.
				59	72	64	23	16.8
	10	8	3	64	58	64	43	13.8
				53	53		50	12.7
	11	2		69	69	72	27	16.2
			8		83	83		8
			3	79	78		15	18.1
			4	87	88	90	1	20.3
			5	77	76	76	17	17.7
			10	64	62		37	14.6

Months. N, S.	Days.	Hours.		Deg. <i>Fal.renh.</i>			R.Socet. Therm.	Edinb. Therm.	
		<i>A. Mer.</i>	<i>P. Mer.</i>	Officers Tent.	Open Air.	Soldiers Tent.		Open	Air.
<i>May</i>	14	8		72	70		26	16.5	
			1	83	84		6	19.5	
			10	78	76		17	17.7	
	18	8		68	68	68	28	16.1	
			1	76	84	83	6	19.5	
			10	80	78	78	15	18.1	
	20	8		64	60	64	40	14.2	
			1	66	65	65	33	15.3	
			5	72	68	74	28	16.1	
			7	64	64	64	34	15.1	
			10	58	57	57	45	13.7	
	21	8		70	72		23	16.8	
			1	82	80		12	18.6	
			11	67	68		28	16.1	
	22	8		70	72		23	16.8	
			1	88	86		4	10.9	
			11	56	55		47	13.3	
	23	8		72	72		23	16.8	
			1	66	64		35	15.1	
			11	56	55		47	13.3	
	24	8		56	56	56	46	13.4	
			2	65	64	64	35	15.1	
			4	54	54	54	48	13.	

Months.

Months. N. S.	Days.	Hours.		Deg. <i>Fahrenh.</i>			Therm.	R. Societ. Therm.	Edinh. Therm.
		A. Mer.	P. Mer.	Officers Tent.	Open Air.	Soldiers Tent.			
<i>May</i>	24		11	48	48	+8	57	11.7	
	25	7		56	54		48	13.	
			I	56	55		47	13.3	
			11	49	48		57	11.7	
	26	8		60	68		29	16.	
			I	64	70		26	16.5	
			10	57	56		46	13.4	
	27	8		57	59		42	14.1	
			I	73	72		23	16.8	
			11	61	60		40	14.3	
	28	7		72	74	74	20	17.2	
			I	83	85	90	5	19.6	
			11	59	59	59	42	14.1	
	29	8		79	79		13	18.5	
		11		92	86	94	4	19.9	
			I	95	92	98	5	21.2	
			10	79	66	67	32	15.5	
	30	8		87	85		5	19.6	
			2	96	94	101	8	21.5	
			10	74	72	72	23	16.8	
	31	7		73	73		22	17.	
			I	84	80		12	18.6	
			10	69	69	69	28	16.2	

Months,

Months. N. S.	Days.	Hours.		Deg. <i>Fahrenh.</i>			R. Societ. Therm.	E. and Therm.
		A. Mer.	P. Mer.	Officers Tent.	Open Air.	Soldiers Tent.		
							Open	Air.
<i>May</i> <i>June</i>	31		11	69	63		36	14.8
	1	7		73	72		23	16.8
			2	92	86	94	4	19.7
			10	72	70	70	26	16.5
	2	8		88	88		1	20.3
			2	96	94	100	8	21.6
			10	72	70	70	26	16.5
	3	8		70	72		23	16.8
			2	79	78		15	18.1
			11	64	60		40	14.3
	4	7		64	64		35	15.1
			1	71	70	81	26	16.5
			11	62	62	62	37	14.6
	5	8		64	64		35	15.1
			1	74	68		29	16.
			10	62	62		37	14.6
	6	5		56	56	58	46	13.4
			2	88	84	94	6	19.5
	8	7		78	72	78	23	16.8
			1	88	79	90	13	18.5
			5	94	80	92	12	18.6
			10	64	62	62	37	14.6
	9	9		81	84	84	15	18.1

Months.

Months. N. S.	Days.	Hours.		Deg. <i>Fahrenheit</i> .			<i>Edinb.</i> Therm.	
		<i>A. Mer.</i>	<i>P. Mer.</i>	Officers Tent.	Open Air.	Soldiers Tent.	R. Societ. Therm.	Open Air.
<i>June</i>	9		4	88	82	90	9	19.
			10	64	62	62	37	14.6
	10	7		78	76		18	17.7
	11	9		64	61	62	39	14.4
			1	72	70	76	26	16.5
			10	64	62	62	37	14.6
	12	8		64	62		37	14.6
			2	67	66		32	15.5
			10	64	63		36	14.8
	13	8		67	66		32	15.5
			1	85	82	90	9	19.
	14	1		64	62	62	37	14.6
		9		65	65		33	15.3
			1	92	88	95	1	20.3
			11	66	66	66	32	15.5
	15	8		64	64	67	35	15.1
			1	89	88	92	1	20.3
			10	64	62	62	37	14.6
	16	8		64	64		35	15.1
			1	74	74		20	17.4
			10	64	64		37	14.6
	17	5		60	58		43	13.8
			1	72	72		23	16.8

Months.

Months. N. S.	Days.	Hours.		Deg. Fahrenh.			R. Societ Therm.	Lamb. Therm.	
		A. Mer.	P. Mer.	Officers Tent.	Open Air.	Soldiers Tent.		Open	Air.
<i>June</i>	17		11	66	64		35	15.1	
	18	8		88	88	91	1	20.3	
			2	98	95	100	10	21.7	
			10	88	88	88	1	20.3	
	20	8		86	82		9	19.	
			1	94	92	97	5	21.2	
			10	76	76		18	17.7	
	21	8		87	86		4	19.9	
			1	96	94	98	8	21.6	
			10	87	80		12	18.6	
	22	8		88	82		9	19.	
			1	100	92	100	5	21.2	
			11	84	84		6	19.5	
	23	7		82	84	87	6	19.5	
		9		97	90	99	2	20.6	
			2	96					
			10	88	84	84	6	19.5	
	24	7		87	84		6	19.5	
			1	90	86	93	4	19.9	
			2	94	87	97	3	20.1	
			4	72	70	70	26	16.5	
	25	2		64					
		7		70	70		26	16.5	

Months.

Months. N. S.	Days.	Hours.		Deg. <i>Fahrenheit</i> .			R. Societ. Therm.		<i>Ethio.</i> Therm.
		<i>A. Mer.</i>	<i>P. Mer.</i>	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.		
<i>June</i>	25		1	88	84	90	6	19.5	
			10	74	72	72	23	16.8	
	26	7		70	68	68	29	15.9	
			1	76	72	78	23	16.8	
			11	70	70		26	16.5	
	27	7		90	87	92	3	20.1	
			1	96	90	99	2	20.6	
			4	73	70	72	26	16.5	
			10	73	68	68	29	15.9	
	28	6		67	64		34	15.1	
			1	90	87	92	2	20.	
			10	78	74	74	21	17.2	
<i>July</i>	2	6		61	60	61	40	14.2	
			1	72	70	70	26	16.5	
			11	64	62		37	14.6	
	5	8		67	64		34	15.1	
			2	79	80	76	12	18.6	
			10	64	62		37	14.6	
	8	5		63	60	62	40	14.2	
			2	100	90	102	2	20.7	
			11	67	61	63	39	14.4	
	12	6		72	70	70	26	19.5	
			1	82	76	84	18	17.7	



Months. N. S.	Days.	Hours.		Deg. <i>Fahrenh.</i>			R. Societ. Therm.	Edinb. Therm.
		<i>A. Mer.</i>	<i>P. Mer.</i>	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
<i>July</i>	12		11	69	62	63	37	14.6
	13	7		72	64		34	15.1
			1	83	73		19	17.5
			10	69	61		39	14.4
	16	7		67	63	66	36	14.8
			1	88	82	94	9	19.
			11	74	67	70	30	15.7
	19	8		72	68	68	29	16.
			2	72	66	67	32	15.5
			8	69	64	64	34	15.1
			11	58	54	57	48	13.
	23	6		68		78		
		11		72		88		
			10	64		60		
	28	8		72		79		
			1	78		93		
			11	70		66		
	30	7		67		72		
		11		80		98		
			11	70		67		
<i>August</i>	2	7		64		59		
			1	73		86		
			11	66		56		

Months.

Months. N. S.	Days.	Hours.		Deg. <i>Fahrenh.</i>			Therm.	R. Societ.	Edinb. Th-rm.
		A. Mer.	P. Mer.	In a House.		Soldiers Tent.			
<i>August</i>	4	8		69		69			
			I	75		97			
	5	6		68		70			
			I	78		92			
			II	73		64			
	7	6		68		61			
			2	69		60			
			II	62		60			
	8	7		69		70			
			I	80		96			
			IO	74		66			
	12	5		63		61			
			I	74		86			
			IO	70		66			

From *July* 23d, the Observations were taken in a House, and a Soldier's Tent.

IV. *A general Method for exhibiting the Value of an Algebraic Expression involving several Radical Quantities in an Infinite Series: Wherein Sir Isaac Newton's Theorem for involving a Binomial, with another of the same Author, relating to the Roots of Equations, are demonstrated.* By T. Simpson F. R. S.

Read Jan. 10.  
1750-1.

**A**MONG all the great improvements, which the art of computation hath in these last ages received, the method of series may be justly one of the most considerable; since not only the doctrine of chances and annuities, with some other branches of the mathematics, depend almost intirely thereon, but even the business of fluents, of such extensive use, would, without its aid and concurrence, be quite at a stand in a multitude of cases, as is well known to mathematicians.

It is for this reason, that the celebrated binomial theorem, for converting radical quantities into series's, is ranked, by many, among the principal discoveries of its illustrious author; seeing, by means thereof, a vast number of fluents are found, that would otherwise be impracticable: nor is there any case, however complex, to which it may not be extended.

It is true, when two or more compound radical quantities are involved together, the operation, by having two or more series's to multiply into one  
another,

another, becomes very troublesome and laborious; and, what is worse, the Law of continuation, whereby a part of the labour might be avoided, is exceedingly hard, if not impossible, this way to be discovered. In the following paper something is attempted towards obviating the said inconveniencies; but whether the success has been answerable, I shall not take upon me to determine.

# PROBLEM I.

To find a series exhibiting the value of  $1 + \frac{x}{a} \Big| ^m$

$\times 1 + \frac{x}{b} \Big| ^n \times 1 + \frac{x}{c} \Big| ^p \times 1 + \frac{x}{d} \Big| ^q$  &c. in simple terms;  $x$  being indeterminate, and  $a, b, c, d, m, n, p, \&c.$  any given numbers, whole or broken, positive or negative.

Put  $u = 1 + \frac{x}{a} \Big| ^m, w = 1 + \frac{x}{b} \Big| ^n, y = 1 + \frac{x}{c} \Big| ^p, z = 1 + \frac{x}{d} \Big| ^q$  &c.

Also let  $\Delta = u w y z, \&c.$  (= the quantity proposed)

Then, in fluxions,  $\dot{\Delta} = \dot{u} w y z, \&c. + u \dot{w} y z, \&c. + u w \dot{y} z, \&c. + u w y \dot{z}, \&c. \&c.$  Which equation, divided by the preceding one, gives

$$\frac{\dot{\Delta}}{\Delta} = \frac{\dot{u}}{u} + \frac{\dot{w}}{w} + \frac{\dot{y}}{y} + \frac{\dot{z}}{z} \&c.$$

But,

But, since  $u = 1 + \frac{x}{a}$ , we have  $\dot{u} = m \dot{x} \times 1 + \frac{x}{a} \dot{m}$  ;

and therefore  $\frac{\dot{u}}{u} = \frac{m \dot{x}}{a} \times \left( 1 + \frac{x}{a} \right)^{-1} = \frac{m \dot{x}}{a} \times$

$1 - \frac{x}{a} + \frac{x^2}{a^2} - \frac{x^3}{a^3} + \frac{x^4}{a^4} \&c.$  by Division.

And in the same manner it appears, that  $\frac{\dot{v}}{v} = \frac{n \dot{x}}{b} \times 1 - \frac{x}{b} + \frac{x^2}{b^2} \&c. \&c.$

Hence, our equation, by substituting these values, becomes

$$\frac{\dot{\Delta}}{\Delta} = \dot{x} \times \left\{ \begin{array}{l} \frac{m}{a} - \frac{m x}{a^2} + \frac{m x^2}{a^3} - \frac{m x^3}{a^4} \&c. \\ \frac{n}{b} - \frac{n x}{b^2} + \frac{n x^2}{b^3} - \frac{n x^3}{b^4} \&c. \\ \frac{p}{c} - \frac{p x}{c^2} + \frac{p x^2}{c^3} - \frac{p x^3}{c^4} \&c. \\ \&c. \quad \&c. \quad \&c. \quad \&c. \end{array} \right\}$$

$$\text{Put } P = \frac{m}{a} + \frac{n}{b} + \frac{p}{c} + \frac{q}{d} \&c.$$

$$Q = \frac{m}{a^2} + \frac{n}{b^2} + \frac{p}{c^2} + \frac{q}{d^2} \&c.$$

$$R = \frac{m}{a^3} + \frac{n}{b^3} + \frac{p}{c^3} + \frac{q}{d^3} \&c.$$

Then it will be

$$\frac{\dot{\Delta}}{\Delta} = \dot{x} \times P - Qx + Rx^2 - Sx^3 + Tx^4 - Vx^5 \&c.,$$

Assume

Assume  $\Delta = A + Bx + Cx^2 + Dx^3 + Ex^4, \&c.$   
let this value, with that of  $\Delta$ , be substituted in the  
last equation: from whence, by comparing the ho-  
mologous terms, there will come out

$$B = PA$$

$$C = \frac{PB - QA}{2}$$

$$D = \frac{PC - QB - RA}{3}$$

$$E = \frac{PD - QC + RB - SA}{4}$$

$$F = \frac{PE - QD + RC - SB + TA}{5}$$

$$G = \frac{PF - QE + RD - SC + TB - VA}{6}$$

$\&c.$

Where the law of continuation is manifest, and  
where it is also evident, that the value of ( $A$ ) the  
first term of the required series, must be an unit;  
because, when  $x=0$ , then the given expression be-

comes  $1^m \times 1^n \times 1^p = 1, \quad Q.E.I.$

# COROL. I.

If  $a$  be taken  $= 1$ , and  $n, p, q, \&c.$  each  $= 0$ ;  
then will  $P = m, Q = m, R = m, \&c.$  And there-  
fore

$$A = 1$$

$$B = m$$

$$2C = mB - mA$$

3D

$$3D = mC - mB + mA = mC - 2C$$

$$4E = mD - mC + mB - mA = mD - 3D$$

$\&c.$

$$\text{Consequently } C = \frac{m \cdot \overline{m-1}}{2}, D = \frac{C \times \overline{m-2}}{3} =$$

$$\frac{\overline{m \cdot m-1} \cdot \overline{m-2}}{2 \cdot 3}, E = \frac{E \times \overline{m-3}}{4} = \frac{\overline{m \cdot m-1} \cdot \overline{m-2} \cdot \overline{m-3}}{2 \cdot 3 \cdot 4}$$

$\&c.$

$$\text{Hence, in this case, } 1 + mx + \frac{m \cdot \overline{m-1}}{2} x^2 +$$

$$\frac{\overline{m \cdot m-1} \cdot \overline{m-2}}{2 \cdot 3} x^3 \&c. (= A + Bx + Cx^2 \&c.) =$$

$\overline{1+x}^m$  : which series is the same with that given by Sir Isaac Newton.

# COROL. 2.

If  $a$  be taken  $= \frac{1}{a}$ ,  $\beta = \frac{1}{b}$ ,  $\gamma = \frac{1}{c}$ ,  $\&c.$  and  $z = \frac{1}{x}$  then will the proposed expression be transformed to

$$\overline{1 + \frac{\alpha}{z}}^m \times \overline{1 + \frac{\beta}{z}}^n \times \overline{1 + \frac{\gamma}{z}}^p \times \overline{1 + \frac{\delta}{z}}^q \&c.$$

$$\text{Also } P = m\alpha + n\beta + p\gamma + \&c.$$

$$Q = m\alpha^2 + n\beta^2 + p\gamma^2 + \&c.$$

$$R = m\alpha^3 + n\beta^3 + p\gamma^3 + \&c.$$

$\&c.$

$$\text{And consequently } \overline{1 + \frac{\alpha}{z}}^m \times \overline{1 + \frac{\beta}{z}}^n \times \overline{1 + \frac{\gamma}{z}}^p \times \overline{1 + \frac{\delta}{z}}^q \&c.$$

$\phi c. = A + \frac{B}{z} + \frac{C}{z^2} + \frac{D}{z^3} \phi c.$  where  $A=1$ ,  $B=PA$ ,  $C=\frac{PB-QA}{2} \phi c.$  as before. Which equation or theorem answers in case of a descending series.

COROL. 3.

Hence, if each of the quantities  $m, n, p, \phi c.$  be taken equal to unity, and their number be denoted by  $v$ ; then will  $1 + \frac{\alpha}{z} \times 1 + \frac{\beta}{z} \times 1 + \frac{\gamma}{z} \times 1 + \frac{\delta}{z} \phi c.$   
 $= A + \frac{B}{z} + \frac{C}{z^2} + \frac{D}{z^3} \phi c.$  Which equation, multiplied by  $z^v$ , gives  $\overline{z+\alpha} \times \overline{z+\beta} \times \overline{z+\gamma} \times \overline{z+\delta} \phi c.$   
 $= Az^v + Bz^{v-1} + Cz^{v-2} + Dz^{v-3} \phi c.$

Whence it appears, that  $\overline{z-\alpha} \times \overline{z-\beta} \times \overline{z-\gamma} \times \overline{z-\delta} \phi c.$  is  $= Az^v - Bz^{v-1} + Cz^{v-2} - Dz^{v-3} \phi c.$   
 Where  $A=1$ ,  $B=PA$ ,  $C=\frac{PB-QA}{2}$ ,  $D=\frac{PC-QB+RA}{3}$ ,  $\phi c.$  (as before);  $P$  being in this case = sum of all the quantities  $\alpha, \beta, \gamma, \delta, \phi c.$   
 $Q$  = the sum of all their squares;  $R$  = the sum of their cubes,  $\phi c. \phi c.$

COROL. 4.

Since  $\alpha, \beta, \gamma, \delta, \phi c.$  are the roots of the equation,  $z^v - Bz^{v-1} + Cz^{v-2} - Dz^{v-3}, \phi c. = 0$ ; it follows,



follows, that, if  $B, C, D, E, \&c.$  be given; the sum of those roots ( $P$ ); the sum of their squares ( $Q$ ), and the sum of their cubes ( $R$ )  $\&c.$  will also be given from the foregoing equations: whence will be had

$$P=B$$

$$Q=+PB-2C$$

$$R=-PC+QB+3D$$

$$S=+PD-QC+RE-4E$$

$$T=-PE+QD-RC+SB+5F$$

$$\&c. \qquad \&c.$$

where the law of continuation is obvious.

These values are the same with those given (without demonstration) by Sir Isaac Newton, in his Universal Arithmetic, for finding when some of the roots of an equation are impossible.

## PROBLEM II.

To find a series expressing the value of  $1 + \sqrt[n]{\frac{x}{a}}$

$$\times 1 + \sqrt[m]{\frac{x^2}{b}} \times 1 + \sqrt[p]{\frac{x^3}{c}} \times 1 + \sqrt[q]{\frac{x^4}{d}} \ , \ \&c.$$

By putting  $u = 1 + \sqrt[n]{\frac{x}{a}}$  ,  $w = 1 + \sqrt[m]{\frac{x^2}{b}}$  ,  $\&c.$  ; and

proceeding as in the last problem; there will be had

$$\frac{u}{u} = \frac{mx}{a} \times 1 - \frac{x}{a} + \frac{x^2}{a^2} - \frac{x^3}{a^3} \ \&c.$$

$$\frac{w}{w} = \frac{2uxx}{b} \times 1 - \frac{x^2}{b} + \frac{x^4}{b^2} - \frac{x^6}{b^3} \ \&c.$$

Whence,

Whence, making  $P = \frac{m}{a}$ ,  $Q = \frac{m}{a^2} - \frac{2n}{b}$ ,  $R = \frac{m}{a^3} + \frac{3p}{c}$ ,  $S = \frac{m}{a^4} + \frac{2n}{b^2} - \frac{4q}{d}$ ,  $T = \frac{m}{a^5} + \frac{5r}{e}$ , &c. and assuming  $A + Bx + Cx^2 + Dx^3 + Ex^4$ , &c. to express the series sought, the several values of  $A, B, C, D$ , &c. will be exhibited by the very equations brought out in the resolution of the preceding problem.

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V. *A Letter from George Bayly M. D. of Chichester, to Henry Pemberton M. D. F. R. S. &c. of the Use of the Bark in the Small-Pox.*

Dear Sir,

Read Jan. 10.  
1750.

THE case I lately mention'd to you in conversation, of which you desired a more particular account, is, as far as I have been able to recollect at this distance of time, as follows.

The patient, a gentlewoman of a fat corpulent habit, and healthy constitution, but 73 years of age, was, on the 6 day of December 1742, seiz'd with the common symptoms of a fever, attended with a sudden great loss of strength; so that, being carried to bed, she was not able to sit upright in it for the least space of time, without being held up by her assistant.

She became afterwards delirious, and on the 4th day vomited much, and pustules of the small-pox appeared, which gradually became more numerous, and increased in bigness. The pustules however were large, distinct, and not in great number. She went on well enough for three days from the first eruption; the vomiting quickly ceased, and we began to promise ourselves that all danger would soon be over: but on the 8 day the pustules were at a stand, and every thing went wrong: whereupon I order'd vesicatories to be applied to the arms, and warm cordial medicines in good doses to be given, and often repeated, in order to promote the growth of the pustules, and bring them to maturation.

We proceeded two days in this method without effect; on the contrary she grew worse; and on the 10th day the pustules were every-where sunk, and in the face appeared quite dry and shriveled; she swallowed with extreme difficulty, had no remaining strength, and seemed to every one about her past all hopes of recovery: I myself thought she would not live a day, or scarce 12 hours, longer. The apothecary, who was her son, seeing the extreme danger of his mother, and how ineffectual my endeavours had been to raise the pustules, desired to know, if I could think of any thing farther to answer this purpose? I proposed to try, if he pleased, the *Cortex Peruvianus*, but without taking upon me to answer for the event. He readily consenting, I prescribed as follows:

*Applicentur Empl. Vesicator. Tibiis internis.*

*R. Cort.*

℞ *Cort. Peruv. pulv.* ʒ℥. *Serpent. Virgin.* gr. iij.  
*Aq. Laët. alexit.* ʒ℥. *Pæon. c.* ʒij. *Syr. Pæon.*  
*mar.* ʒiij. *M. f. haust. mox exhibend. et omni tri-*  
*horio repetend.*

Soon after taking the second draught she was plainly alter'd for the better; in 8 hours very much amended; and within 24 hours was freed from the most dangerous symptoms; the pustules, which had been sunk and wither'd, gradually rising nearly from the time of taking the bark.

She persisted in taking the draughts from the 10 to the 15 day, and took 28 in all; by the use of which the first pustules arrived in five days to perfect fulness and maturation; and a second eruption of pustules succeeded, which render'd her blind, and were so very numerous, that the matter of them almost every-where ran together, and formed large abscesses underneath in various places: and though the discharge from the ulcers was very great, yet (as if this were not sufficient to depurate the blood) there arose all over the body a great number of boils, inasmuch that the whole surface of the body was, as it were, one continued ulcer; to cover and defend which, three whole sheep-skins were employed spread with *Cerat. de Lap calamin.* and daily renew'd for a long time; it being two or three months, before all the ulcers were cicatrized.

Besides the above-mention'd effects of the bark, it was observed greatly to invigorate and fortify her spirits, during the time of giving it, which was no longer than what I thought sufficient to bring the  
 matter

matter of the pustules to a proper digestion. But this was not long enough to prevent a second fever, which, I am inclined to believe, the continued use of the bark would have done, or at least have much abated and shorten'd it. However, to guard against this fever, on the 15 day 8 ounces of blood were taken away: she was likewise purged on the 16, 18, and 22. But, notwithstanding this method of bleeding and purging, as far as her strength would admit, the fever came on and increased.

I then tried her with 3 or 4 draughts of bark: but the fever not immediately giving way to this, I was afraid to proceed with it farther at that time. But the fever at length being attended with a *Coma*, and other dangerous symptoms, made me again doubt of her recovery.

This determin'd me to try the bark in earnest against the fever, the extraordinary effects of which I had already experienced in the preceding stage of the disease. I therefore order'd a strong decoction of *Cort. Peruv. Serpentar. Virgin. Croc. Coccinel.* which she continued to take once in three or four hours for 24 days together; during which time she took 17 ounces of bark, and was thereby freed from the fever intirely. After leaving off the bark, she took 11 purges at proper intervals, and then left off taking any more medicines, being recovered to a more perfect health than before her illness, and so continued.

Give me leave to subjoin the case of a healthy young man, who, in July 1746, had the small-pox by inoculation. The eruption came on at the right time;

time; but, three or four days after, in dressing the incisions, three or four purple spots were observed about them, which occasioned my being called in. I took notice, that the pustules, which were very numerous, were here and there livid, and in the arms and thighs of a dark colour, tending towards a mortification. Whereupon I immediately prescribed ʒss. of bark to be given, and repeated once in three hours; which was accordingly done for eleven days successively; during which time he took 47 doses of bark, viz. in all, 3 ounces wanting half a drachm. It was really wonderful to see, how soon the bark alter'd the colour of the pustules, brought them on to digestion, supported the patient's strength, prevented a second fever, and carried him thro' the disease without the least difficulty, or bad symptom.

I am, &c.

VI. *A Method of making artificial Magnets without the Use of natural ones; communicated to the Royal Society by John Canton, M. A. & F. R. S. To which is prefixed the President's Report.*

Read Jan. 17.  
1750.

**A**T a meeting of the Royal Society on Thursday the 17 day of January 1750, the President acquainted the gentlemen there present, that Mr. John Canton, one of their members, who had for a considerable time, and with great diligence,

diligence, applied himself to the making of philosophical experiments of various sorts, had, among others, attempted to convey a considerable magnetic virtue to bars of hardened steel; and that having therein so well succeeded, as to be able to impregnate such bars with this virtue, to as high a degree at least, as any of the same weight and dimensions, which he had yet seen or heard of; and to as high a degree, as he apprehended the same bars were, in their present state, capable of being impregnated; he was thereupon ready at that time, and prepared, to lay some of his experiments to this purpose before the gentlemen of the Society, and to shew them the whole method and process of his operation: whereby he could, in about half an hour's time, communicate to six bars of hardened steel, at first entirely destitute of any magnetic virtue whatsoever, the utmost virtue they were capable of receiving; and that without the mediation or assistance of any natural loadstone, or of any artificial magnet, to which any virtue had before purposely and previously been conveyed.

The president then delivered to the secretary the following paper, containing, in Mr. Canton's own words, the whole description of his process, with the directions he had drawn up, whereby any other person might readily perform the same. After which Mr. Canton immediately exhibited the main experiment itself, as described in his paper, together with some others: all which succeeded greatly to the satisfaction of the company. But as he feared himself, that he should not be able, by reason of the concern he was under in the presence of so many worthy persons, and for whom he had so great a respect,  
 either

either to make his experiments to so good an advantage, as they might otherwise be made, or to give to his bars the same degree of strength, which he had formerly and frequently given to others of the same sort ; he was therefore desirous to refer himself for such particulars, to what the president of the Society had already seen and taken minutes of, a few days before ; and who thereupon reported, as he said he could faithfully do, to the best of his judgment and observation, the following facts :

That having been in company with Mr. John Ellicot, of the Society, at Mr. Canton's house in Spital-square, Bishopsgate-street ; he had there seen him communicate the magnetic virtue, in the manner described in his paper, to six bars of the dimensions therein mention'd, and weighing, one with another, about an ounce and three quarters each, Troy weight. That these bars were at first perfectly indifferent as to either end of a compass needle, but that they did any of them, after their impregnation, lift by one of their ends, strongly and distinctly, full twenty-eight troy ounces ; the whole operation of giving them their virtue having taken up nearly thirty minutes.

That Mr. Canton had besides shewn him at the same time two larger bars, each half an inch square, ten inches and an half in length, and weighing nearly ten ounces and twelve pennyweight : and that these, as he was informed, had been, *mutatis mutandis*, impregnated in the same manner as the former. That he had not indeed himself seen their virtue communicated to these bars, but that he had seen a trial made of their strength, by which one of them had lifted



in his presence, by one of its ends, seventy-nine ounces and nine pennyweight.

That he had also been shewn a flat semicircular steel magnet, weighing an ounce and thirteen pennyweight: and that the same had lifted before him, by applying its two ends together to an iron wedge, ninety troy ounces.

That he had likewise been told by Mr. Canton at the same time, in what manner the virtue might readily be taken away from any of his bars, which experiment he had also seen him put in practice. And that Mr. Canton had moreover changed in his presence the poles of a natural loadstone, by placing it in an inverted direction, between the contrary poles of two of his larger bars, laid down at some distance from each other, in the same strait line continued: and that he had even performed this, without touching the stone with either of the bars, and only by placing it, in the manner just mentioned, between their poles, at the distance of about a quarter of an inch from either of them.

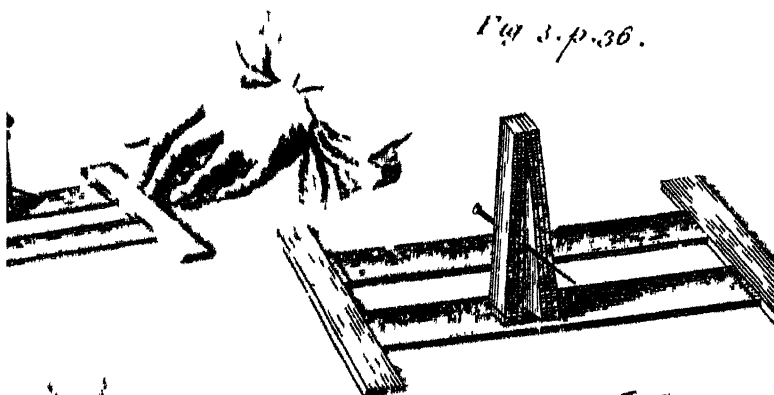
*A Method of making Artificial Magnets  
without the use of, and yet far superior to,  
any natural ones.*

**P**ROcure a dozen bars; six of soft steel, each three inches long, one quarter of an inch broad, and one-twentieth of an inch thick, with two pieces of iron, each half the length of one of the bars, but of the same breadth and thickness; and six of hard steel,  
each

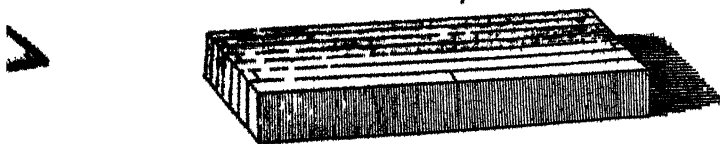




*p. 35.*



*Fig. 6. p. 38.*





each five inches and an half long, half an inch broad, and three-twentieths of an inch thick, with two pieces of iron of half the length, but the whole breadth and thickness of one of the hard bars: and let all the bars be marked with a line quite round them at one end.

Then take an iron poker and tongs \* (Tab. II. Fig. 1.) the larger they are, and the longer they have been used, the better; and fixing the poker upright between the knees, hold to it near the top one of the soft bars, having its marked end downward, by a piece of sewing filk, which must be pulled tight with the left hand, that the bar may not slide: then grasping the tongs with the right hand a little below the middle, and holding them nearly in a vertical position, let the bar be stroked by the lower end, from the bottom to the top, about ten times on each side, which will give it a magnetic power sufficient to lift a small key at the marked end: which end, if the bar was suspended on a point, would turn toward the north, and is therefore called the north pole, and the unmarked end is, for the same reason, called the south pole of the bar.

Four of the soft bars being impregnated after this manner, lay the other two (Fig. 2.) parallel to each other, at the distance of about one-fourth of an inch, between the two pieces of iron belonging to them, a north and a south pole against each piece of iron: then take two of the four bars already made magnetical, and place them together, so as to make a

E 2

double

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\* Or two bars of iron.

double bar in thickness, the north pole of one, even with the south pole of the other; and the remaining two being put to these, one on each side, so as to have two north and two south poles together, separate the north from the south poles at one end by a large pin, and place them perpendicularly with that end downward, on the middle of one of the parallel bars, the two north poles towards its south, and the two south poles towards its north end: slide them backward and forward three or four times the whole length of the bar, and removing them from the middle of this, place them on the middle of the other bar as before directed, and go over that in the same manner; then turn both the bars the other side upward, and repeat the former operation: this being done, take the two from between the pieces of iron, and placing the two outermost of the touching bars in their room, let the other two be the outermost of the four to touch these with: and this process being repeated till each pair of bars have been touched three or four times over, which will give them a considerable magnetic power, put the half dozen together after the manner of the four (Fig. 3.) and touch with them two pair of the hard bars, placed between their irons at the distance of about half an inch from each other: then lay the soft bars aside; and with the four hard ones let the other two be impregnated (Fig. 4.) holding the touching bars apart at the lower end near two tenths of an inch, to which distance let them be separated after they are set on the parallel bar, and brought together again before they are taken off: this being observed,

observed, proceed according to the method described above, till each pair have been touched two or three times over. But as this vertical way of touching a bar will not give it quite so much of the magnetic virtue as it will receive, let each pair be now touched once or twice over, in their parallel position between the irons (Fig. 5.) with two of the bars held horizontally, or nearly so, by drawing at the same time the north of one from the middle over the south end, and the south of the other from the middle over the north end of a parallel bar; then bringing them to the middle again without touching the parallel bar, give three or four of these horizontal strokes to each side. The horizontal touch, after the vertical, will make the bars as strong as they can possibly be made: as appears by their not receiving any additional strength, when the vertical touch is given by a greater number of bars, and the horizontal by those of a superior magnetic power. This whole process may be gone thro' in about half an hour, and each of the larger bars, if well-hardened \*, may be made to lift twenty-eight troy ounces, and sometimes more. And when these bars are thus impregnated, they will give to an hard bar of the same size, its full virtue  
in

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\* The smith's manner of hardening steel, whom I have chiefly employed, and whose bars have constantly proved better than any I could meet with beside, is as follows: having cut a sufficient quantity of the leather of old shoes into very small pieces, he provides an iron pan, a little exceeding the length of a bar, wide enough to lay two side by side without touching each other or the pan, and at least an inch deep. This pan he nearly half-fills with the bits of leather, upon which he lays the two bars, having fastened to the  
end



in less than two minutes: and therefore will answer all the purposes of magnetism in navigation and experimental philosophy, much better than the loadstone, which is well known not to have sufficient power to impregnate hard bars. The half dozen being put into a case (Fig. 6.) in such a manner, as that two poles of the same denomination may not be together, and their irons with them as one bar, they will retain the virtue they have received: but if their power should, by making experiments, be ever so far impaired, it may be restored without any foreign assistance in a few minutes. And if, out of curiosity, a much larger set of bars should be required, these will communicate to them a sufficient power to proceed with, and they may in a short time, by the same method, be brought to their full strength.

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end of each a small wire to take them out by: he then quite fills the pan with the leather, and places it on a gentle flat fire, covering and surrounding it with charcoal. The pan being brought to somewhat more than a red heat, he keeps it so about half an hour, and then suddenly quenches the bars in a large quantity of cold water.

VII. Aurora borealis, observata a *Petro Gabrij*, J. V. D. Phys. Astron. et Math. anni 1750, die 27 *Februarii*, Nov. St. *Hagæ Com.*

Read Jan. 24.

1750.

**O**BSE RVAVI tempore vespertino per-  
rarum meteoron, quæ mihi aurora  
borealis visa, referens magnam lucem, eamque for-  
mam iridis, principium sumens ab horizonte circa  
orientem, finiensque ad horizontem circa occasum.  
Culmen erat versus meridiem zenith, et fere 80 grad.  
supra horizontem; (Tab. I. Fig. 2.) latitudo autem  
prope verticem fere 2 grad. ad utramque extremita-  
tem pergens quasi cuspidatim. Medius arcus magnam  
candidamque lucem emittebat, quæ tamen ad limbos  
et magis debilis et subcœrulea apparebat.

Meteoron hoc decima vespertina maxime vividum  
conspicere, at vero post horæ quadrantem discussum  
erat. Quum autem sidera noctem belle illustrabant,  
distincte dabatur hæc, quanquam debilius, quam quæ  
extra arcum, transpicere.

VIII. *Some farther Observations on the Cancer major; communicated in a Letter to Mr. Kleine, Secretary of Dantzick, by Mr. Peter Collinson, F. R. S.*

My dear friend,

London, Jan. 1. 1750.

Read Jan. 24.  
1750.

AS you seem to doubt, that crabs and lobsters cast or shed their shells, tho' I am certain it is fact and truth, I am desirous you should be satisfied from undoubted accounts, which I have procured from my cousin Cooke, who lives in the Isle of Wight, where crabs are in great plenty, and the fishermen very honest people, whom he has known many years, and from whom and his own observations is collected the following account.

That the *cancer major*, and all species of crabs, cast their shells, is certain; but at what season of the year, or how frequently, is not exactly to be determined; but it is believed to be annually at the beginning of the summer, sooner or later, according to the greater or lesser strength of the crab.

If you observe the shell of this creature, you will see in the under part a future in the form of a crescent, which retains a part of the shell of the same figure. At the time of casting the old shell, this future opens, and leaves a space sufficient for drawing out the whole body; after which the *thorax* drops its breast-plate, and then the legs quit their crustaceous coverings.

The carcase now is left enveloped with a soft skin like wet parchment. In this helpless state it is incapable

pable of moving, but lies at the bottom of the sea, between the rocks, until its new shell acquires a sufficient hardness and consistence, fit for its defence, and its limbs grow strong enough to bear its weight, and carry it about, to perform its necessary functions; whilst the old shell is left in two parts, that, which cover'd the body, in one, and that, which cover'd the breast and legs, in another.

It happens sometimes, that the shell hardens prematurely. In this case, the poor animal is made a prisoner, being so cramped, that he cannot disengage himself from his hiding-place, till found by the fishermen, and set at liberty by moving the stones from about him.

It is surprising to consider, how a creature can live long confined without any aliment, and yet increase in its dimensions. But that the crab will subsist without a sensible decay in the fishermen's pen-pots \*, for the space of some months, is very certain.

The more healthy and thriving a crab is, the more frequently he casts his shell. But, if he becomes sickly, and wasting, the old shell remains on him, until such time as he recovers strength and vigour to cast it.

When the fishermen take a crab, that is not in a good condition, they return it into the sea, and often mark it on the back with a sharp-pointed iron, or top of a knife; and this mark not only remains on the old shell, as long as it continues on, but is found in the same manner impress'd or ferrated on the new  
F
shell;

\* These are cages in the sea, made with willow-twigs to keep the crabs in.

shell ; a very strange and surprizing phænomenon ; but I am assured it is fact.

If a crab receives a small wound in the very extremity of the claw, he generally bleeds to death, or pines away by slow and insensible leaking of the vital moisture.

But if he receives any considerable wound or hurt, that gives him pain, he instantly throws off the offending member, and all is safe (as I have observed in a former account) and a new limb soon succeeds to make it again perfect. The leg is always thrown off at the same joint ; the blood is stopp'd by the membrane, that lines that articulation, contracting itself in the form of a purse.

If a crab be brought near the fire, he throws off the legs, which feel a painful heat.

In like manner if a crab be thrown into hot water, he casts off all his legs together. For which reason, when they are to be boiled, they put them into the pot in cold water, and let it warm very slowly, until the creature gradually die.

These, my dear friend, are the principal remarkables, relating to this animal ; which being added to those, which some time ago you deliver'd to the Royal Society, and published in their Transactions, will go pretty far in the natural history of this wonderful animal.

I am, my dear friend, with much respect and esteem,

Your affectionate friend,

P. Collinson.

The lobster casts his shell much in the same manner as the river crayfish, which are a species of fresh-water lobsters.

IX. *An Account of the Right Honourable  
Horace Walpole Esq; drawn up by him-  
self* \*.

April 1750.

Read Jan. 24.  
1750.

**A**BOUT eighteen years ago, when his majesty resided at Hampton-Court, I was taken ill there with what was thought to be a fit of the colic only, being subject to that disorder, when I was very young; and the physician treated me accordingly. When some days after I was got perfectly well, in making water one morning I voided a stone in the pot about the bigness of a barley-corn, which without doubt had occasion'd, whilst it lay in the *ureter*, the colical pain, which I had felt. From that time I was frequently troubled with severe fits of the same pain, which lasted, until, by turpentine clysters, and other lubricating medicines, I had brought away a stone.

Being advised at last to drink a pint of whey turn'd with cream of tartar every morning, and having followed that method from the beginning of May to November, at the end of two years, during which time my pains frequently returned and ended in the same manner, I found myself at last perfectly cured; for, having continued to drink the whey yearly, I continued free from those pains, voiding only at

F 2

times

\* The supplement of this case continued till April 1752, will be published in these Transactions, as read before the Society June 4, 1752.

times some red gravel, till 1747. In the spring of that year, whilst I was at a friend's house in town to dine there, having a need to urine, I made, instead of water, what was almost pure blood ; and so from time to time almost all that year, I was often call'd upon to make water by very short intervals, which was more or less discoloured, and seldom very clear, and frequently attended with great pain and some gravel. That whole year, until the next spring, I took variety of things of a lubricating and cooling nature, which it is unnecessary to particularise, without any good effect. The next winter in town, I found I grew daily worse ; and altho' I did not always make bloody or coffee water, yet my provocation to urine was more frequent, which, after an hasty gush of a spoonful of water, suddenly stopp'd, with excessive pain, and it was attended with a *tenesmus*, and an irritation at the end of my yard. Mr. Ranby, the surgeon, and Mr. Graham, the apothecary, having often visited me, and having had constant accounts of my disorder, and the symptoms, that accompanied it, both declared, that there must be a stone in my bladder. I was willing to be probed ; but as I had no thought of being cut, Mr. Ranby declined undertaking that troublesome office, being persuaded, without the trial, that I had a stone in my bladder. Lord Barrington, hearing of my complaint, was so good as to send me, I think, the 5th volume of the *Scots Medical Essays* containing Dr. Whytt's account of the good effect, which taking of soap and lime-water had had in cases similar to mine ; with ingenious reflections and directions relating to that cruel disease, and the remedy for it. I read them with  
great

great satisfaction, and would have immediately fallen into that method ; but my relations, touch'd with the fatal effects, which Dr. Jurin's *lixivium* had had upon the late lord Orford, would not suffer me to follow my own inclinations.

While I had a severe fit upon me, I was visited by the earl of Morton, who, upon hearing what was my disorder, gave me an account of the powerful benefit and entire cure, which Mr. Summers had found in voiding the stone, that had tormented him for many years, by adding lime-water to the soap, which he had taken for some time without any success.

This example, by the encouragement of Mr. Graham, my apothecary, fix'd my resolution to follow that method ; and accordingly before I left the town, I often perus'd Dr. Whytr's essay relating to the stone.

In March 1747-8. I began at first with taking every day half an ounce of Alicant soap, made up into pills with the syrup of marshmallows, and drank upon it about a pint of lime-water made of oyster-shells ; mixing a spoonful of milk with it, and drinking a spoonful after it, to take away the nauseousness of the taste.

Upon the road, as I went into the country in May 1748, I had a most severe fit at Newport, making bloody water, with frequent interruptions at short intervals, attended with violent pains, which continued upon me to such a degree, that I could not endure the horses to go more than a foot-pace for above 70 miles, until I came home.

After my arrival there I was tolerably well for some days ; but the least motion in a coach, or even  
in



in walking, brought the disorder upon me. I was always (which is very remarkable) entirely easy, when I lay in bed, but was obliged, when I got up, to take my couch; and could not venture to move from thence, but upon necessary occasions. In the mean time, I continued to take the soap and lime-water, which by degrees I increased so far, as to take at different times an ounce of soap, and three pints of lime-water, every day, observing a very regular diet. After some months I found myself extremely easy in my ordinary motions; but I never ventur'd to walk far, nor go at all in a wheel-carriage, keeping myself as quiet as I could, until I should be obliged to go to parliament.

Just before I left the country, Mr. Ranby made me a visit; and altho' I had felt no pain nor symptom of my disease for some time, he advised me not to hazard going to town by any means, unless it were in a litter. However, having caused an easy voiture to be made, I undertook the journey in it the 20 of November 1748. which was regulated by the horses going no faster than a gentle walk, and but twenty miles a day.

The cold weather, and the tediousness of creeping so slow, made the coachman sometimes fall into a trot, which I perceived, but finding no inconvenience, did not check his pace. The set stages were observed, but the last two days, and particularly the last day, the coachman drove from Harlow to Whitechapel as full a trot as the horses could well go at any time; and I felt not the least disorder. I took a chair at Whitechapel, and all that winter made use of nothing else, and continued extremely well; but, about two months after my coming to town, I found  
some

some small uneasiness in making water, and in two or three days I voided with my urine something of a flat shape about the bigness of a silver penny, cover'd with a soft white *mucus*, which, when it was dry, was plainly of a stony substance; and after that have never been troubled with the least symptom of that cruel disorder; and I found myself so well in the country last year, that, contrary to the advice of all my friends, I undertook in my coach a journey to Chatsworth in Derbyshire from my house in the country, at least 160 miles, to pay a visit to the Duke of Devonshire, the horses going as round a trot as they could conveniently, according to the road; and the last 10 or rather 15 miles, from Hardwicke to Chatsworth, a most rugged and rocky way, we neither spared ourselves nor our horses; and the great shocks upon the stones broke the springs of my coach, but gave me not the least uneasiness, and I have ever since continued with respect to my former disorder, as well as ever I was in my life; but I have now-and-then voided, after I have sat a great while in the House of Commons, some red gravel.

As I never perceived, that I voided during my illness any flecks of a stone, besides that one, which I have mentioned above, and was never searched by an instrument; I can no otherwise pronounce it to be the stone, unless by the symptoms, which I felt, and the judgment of the surgeon and apothecary, who attended me, from these symptoms. But it is very remarkable, as I said before, that I never felt those symptoms, while I lay in bed, and not to so great a degree upon my couch, as upon my legs; which looks as if the posture made a great alteration. And that methinks could not be the case,

the case, if I had been troubled with only a scorbutic corrosive humour. I must leave it to the learned in physic, to make what conclusions they think fit from this true state of my case. I think I remember in some of Dr. Whytt's observations, that if the medicine would not break or bring away the stone, it might cover it with a soft velvet coat, so as to blunt the edge of it, and keep it from vulnerating any part of the bladder. This may probably be my case, if I have still a stone there; and therefore I continue to take daily a third part of the soap and lime-water, which I used, when I took the full quantity.

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*X. Extract of the Observations made in Italy, by the Abbé Nollet, F. R. S. on the Grotta de Cani. Translated from the French by Tho. Stack, M. D. F. R. S.*

Read Jan. 24. 1750. **T**HIS cavern, known so long a time, and celebrated by so many writers, was probably called La Grotta de Cani, because it is commonly on this species of animals, that experiments are made for the curious, who visit it. It lies in the side of a little hill on the eastern border of the Lago di Agnano, between Naples and Pozzuolo. It is not suffer'd to stand open, but is under the care of a man, who, at about an hundred yards from it, keeps a natural stove \*, that is, a small building, level with the  
the

\* Stove di San Germano.

with the ground, divided into five or six rooms, which are so hot from the nature of the soil, that patients go thither to sweat by order of the physicians.

The grotto, of which I am to give an account, is not dug into a rock, but into a sandy earth, which however is of sufficient tenacity and consistence to keep together without tumbling down, tho' the sides or walls are cut perpendicular. It is somewhat more than three feet wide, near two toises (or twelve feet) long; five or six feet high at the entrance, and a little less than three feet at the inner end.

Tho' the ground is a little sloping from within outward, and much more so from the door to the road, which is about five yards from it, and runs along the foot of the little hill; yet one walks directly into it, as upon level ground, without the assistance of steps to go up or down: which shews, that the slope is pretty even from the bottom or inner end of the grotto to its mouth, and from thence to the road. The knowledge of this particular is necessary for better comprehending what I have to say in the sequel.

When a person places himself at the distance of some few steps withoutside, and stoops so as to have the eye nearly on a level with the ground of the grotto, newly opened, and well illuminated, he sees a vapour within it, pretty much like that, which appears over a chafing-dish of red coals, but with this difference, that it is more sluggish and heavy; for it does not rise above five or six inches high. This fluid, which is hardly visible, and seems so subtil to the eye, spreads regularly, and seems to effect an

æquilibrium, as if it were a liquor: its surface, much better terminated than that of other vapours, balances visibly under the air, as if these two substances were unwilling to intermix.

I entered the grotto, and found the ground moist; and I was assured, that that was its usual state. This moisture is observable likewise all around the sides, to the height of ten inches, and no more. Of this you may easily judge by the colour of the earth, which in that part is browner and softer than anywhere else. And yet this moisture never increases to the degree of forming any drainings, or even the least visible drops. Nor is there any saline efflorescence to be perceived, as is seen on the walls of the stoves above-mentioned. After having stood upright some minutes, I could remark nothing more than a slight earthy smell, like that which commonly prevails in subterraneous places, which have been kept shut. But I felt about my feet a gentle warmth, which seem'd to rise about the same height with the vapour already mentioned. In order to be certain of this, I put down my hand, and had the same sensation as if I had thrust it into the steam of boiling water, at eight or ten inches above the evaporating vessel. From another immersion of my hand, which lasted about a minute, it contracted neither smell nor taste, that I could perceive by applying it to my nose, or laying my fingers on my tongue. A small thermometer, graduated according to M. de Reaumur's scale, which I left on the ground in the grotto for above half an hour, marked 29 degrees above the freezing point. It would probably have risen higher, if the door had not been left open. For, when I made  
this

this experiment, the heat of the exterior air was hardly 18 degrees.

I went out of the grotto, and having knecled down at some few steps distance below the entrance, in order to examine the vapour a second time, I observed its waving motions under the air, better than the first time; because now both these fluids had been put in agitation just before. I had scarcely been some moments in this posture, when I felt in my legs and left-hand, which I had laid on the ground to support me, a heat like that, which I had remark'd in the grotto, but weaker. I retir'd a little sideways, bowing down my head so as to view the surface of the earth almost horizontally, and very distinctly saw a vapour similar to that of the grotto, but not rising so high, and seeming to glide along, and follow the slope of the ground.

Hence I conjectur'd, that this fluid, too heavy to rise more than five or six inches, without being confined on every side, spread itself from the cavern, where its source lay, into the places below it; and that it was dissipated there, either by being divided into a large space, or by yielding to the agitations of the air. I imagined further, that the ground adjacent to the grotto might possibly exhale this fluid, which I perceived, as well as the grotto itself, only with the difference of more or less. The warmth, which I felt in my hand, while I kept it on the ground, render'd the last of these conjectures very probable; and the first was converted into certainty by the following experiment.

It is a constant custom to entertain the curious, who visit the grotto, with a well-lighted flambeau,

which is extinguish'd as soon as it is thrust into the vapour. I made the experiment several times myself, and I always saw the flame perish without noise, without that sort of hissing, which is heard when an ignited body is quenched in water, or any other substance that contains a great deal of water. In examining this phenomenon, I discovered another not less curious. The thick smoke, which appeared immediately after the extinction of the flambeau, remained floating on the vapour; and, being lighter than it, but heavier than the air above it, it spread between both, and moving outward slowly at first, and afterwards quicker, because the slope grew greater, it plainly indicated the motion and direction of the fluid, that carried it along.

If any one ask, why this smoke did not ascend into the air that was over it, and whence proceeded that degree of gravity so unusual to smoke? my answer is, that probably it proceeded from the vapour, in which the flame had been smothered. One may imagine, that these two fluids, being better adapted to mix with one another than with the air, were blended together towards the surface of the vapour; and that the smoke, tho' still the lighter of the two, retained weight enough to remain floating under the surface of the air.

The vapour of the grotto is not the only one, that has been seen moving thus under the air, and spreading from its source into lower places. After great eruptions of Vesuvius, the ditches, cellars, cisterns, and wells, in the neighbourhood of the volcano, and chiefly near the places where the *lava's* stopped, are sometimes found full of a sort of  
*mosfeta*

*profeta* \* or damp, which much resembles that of the grotto, excepting that it is not permanent : but while it lasts, people observe, that, after having filled the place of its source, it overflows, runs into lower grounds, and stops in places, that have any cavities ; as water does, when a basin is too full †.

After the experiment of the flambeau, that of the dog was performed before me. The keeper of the grotto took the two fore legs of the creature in one hand, and the hind legs in the other. He went into the grotto, in the middle of which he laid him upon his side, and held him down : immediately the dog struggled to get loose, or at least to raise his head out of the vapour : he panted, as if his breath failed him ; rattled in the throat and snorted, as if to throw out something, that he did not care to swallow. After being thus tortured for three minutes, his strength failed him, and he lay quite motionless. He was immediately carried into the open air, of which he drew in long draughts, as a person recovering from a fainting fit. In the space of two minutes he was able to get upon his legs, and seemed to be in his natural state. This dog was young, vigorous, of a middle size ; and his master assured me, that he had used him for the like experiments almost every day for above six months past.

I took a cock, and having carried him into the grotto, I plunged his head into the vapour. Scarcely was it in, when he strained to vomit. And indeed, the food, which he had taken some minutes before,

\* *Mephitæ*, a deadly or very dangerous exhalation.

† *Neapol. scient. acad. de Vesuvii conflagratione commentarius*, cap. 6.



before, came up in abundance into his mouth: he was suffocated all at once beyond recovery.

To the same trials I put several frogs successively, just caught on the borders of the lake. In three or four minutes they were all stupefied, and remained almost without motion: but altho' I left them in that condition above a quarter of an hour, they soon recovered upon being removed into the open air.

Large flies, a beetle of that tribe called *scarabæi stercorarii*, and some butterflies, which I treated in the same manner, were longer without giving any signs of their suffering, and they came to life after a syncope of longer duration.

By these two last experiments I found, that reptiles and insects hold out against the effects of the vapour longer than other animals. I contented myself with having observed this twice; because Father La Torre\*, who assisted me in making these experiments, assured me, that he had fully convinced himself of the fact, by a series of experiments, which he had made the preceding year with M. Taitbout, our consul at Naples. And indeed, M. de Reaumur having been pleased, after my return, to give me the result of those same experiments, which had been put into his hands, I saw, that a toad resisted near half an hour; that a lizard was not dead at the end of an hour and a quarter, and that a large grasshopper stir'd in the vapour, after being more than two hours in it.

Wherefore

\* A *Somaschian* frier, professor of philosophy, and correspondent of the academy of sciences.

Wherefore it cannot be doubted, that this vapour is capable of taking away the life of an animal. If experiments had discover'd to us any pestilential quality, any secret poison in it, doubtless we ought, with most authors, who have treated of *moseta's*, to range it among those deadly exhalations, whose bad effects are felt, before they can be foreseen; because they do not strike our senses by any disagreeable smell, or any other quality proper to inspire mistrust. But it is not by the bare extinction of animal life, that a judgment can be formed of them, inasmuch as this effect may equally proceed, either from a substance, that acts by destroying or infecting as a poison; or from a fluid, which takes the place of another, whose functions it is not capable of performing. It is rather by examining the vapour itself, with a view to know its nature, or at least some of its essential qualities; and in this view it was that I prosecuted my experiments.

Having cut a sheet of blue paper in two, I laid one half of it on the ground in the grotto, and let it lie there near half an hour. When I took it out, it was somewhat warm, it had contracted no moisture, and its colour, compared with that of the other half-sheet, which I kept in my pocket, underwent no other change than a slight cast inclining to violet.

I placed a water-glass, with the mouth downward, at the bottom of the grotto, and left it in that situation long enough to have reason to think, that the vapour had well filled it. I then turn'd it, and set it on its bottom, without taking it out of the vapour, and then poured some syrup of violets into it, but I could not perceive any change of colour in the syrup.

The

The effect was the same, when I poured off this same syrup into another glass, upon some of the earth fresh taken from the same spot.

I soaked a linen cloth in very strong vinegar, and having tied it to the end of my cane, I put it into the vapour of the grotto; but tho' I held it there above three minutes, I saw no sign of fermentation.

It came into my mind to try, if the linen of vinegar might not be capable of securing an animal against the ill effects of the vapour. I wrapped the same piece of linen round the mouth of the dog, which had served for the former experiment, and seemed now not to feel any of its effects; I wrapped it, I say, so as that he might breathe freely; and while his master kept him lying down in the grotto, I held a sponge imbibed with vinegar to his nose. But all this did not prevent his having the same symptoms, and in the same space of time, as in the former trial. And he recover'd in the same manner, when he was removed into the open air.

As we had passed part of the day in the *Selfataro*, our shoe-buckles, which were of (*tombac*) yellow metal had considerably changed their colour. I was sorry, that I had not some pieces of the same metal polished, to throw into the vapour of the grotto, in order to see, if we might not discover some arsenical quality in it: but Father La Torre, to whom I intimated my concern, told me, that that was one of the experiments made by M. Tailbout; and that the metal, after a considerable space of time, still appeared of the same colour as before.

A moment afterwards I found on the ground a bit of leaf-brass, which I had made use of above two hours  
before,

before, for some electrical experiments: but either it had not changed colour at all, or the difference, if any, was not discernible.

By these experiments, we do not see positively what this fluid is, which quenches flame, and kills animals in the Grotto de Cani; but in my opinion we learn pretty well what it is not. We may say with great probability, that it is neither sulphureous, nor arsenical, nor alkali, nor acid, to the degree of being dangerous, or of doing sudden mischief by any of these qualities. Besides, it makes no impression on the skin of the hand; which might make one believe, that it would make none on the face, eyes, tongue, or perhaps on the internal parts of the body, if it were convey'd in only by the same ways with the food. But let us not stop at conjectures: here are facts, which answer these questions.

Embolden'd by all the experiments above-recited, and by the inferences, which I drew from them, I thought I should not commit an imprudent action, in plunging myself into the vapour, with the precaution however of not breathing it, and of staying but very little time in it. I kneeled down in the grotto, and leaning both my hands on the ground, I bowed my face forward to within two or three inches of the bottom; keeping my eyes open, my tongue a little way out of my mouth, and holding my breath for a moment.

In this first immersion I felt a touch pretty much like that of boiling water containing some salt; which instantly made me shut my eyes, by a motion natural to that organ, when any thing but quiet pure air strikes it. But it was not attended with any painful

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impression,

impression, or any sort of taste on my tongue, which remain'd uncover'd all the time I held my face down, which was three or four seconds.

The more I studied the vapour of the grotto, the less I found it capable of acting as a poison. I persuaded myself, that one may make an animal swallow some of it with his food, without endangering his life: and to be certain of this, I gave some bread, soaked a long time in the vapour, to a chicken, which eat it without reluctance, and shew'd no signs of being incommoded thereby.

As I was on the point of quitting this famous grotto, never to see it more, in all probability, I was very desirous, that nothing might be forgot, that could be done there. I was resolv'd in particular not to omit certain trials, of which one cannot form a right judgment, without having actually made them; and which I would not afterwards presume to require from the complaisance or zeal of a correspondent. I took a strong fancy to breathe this vapour myself, which had hitherto been one of the chief objects of my inquiries. Doubtless this would have been a blameable rashness two or three hours before: but whosoever will recollect all the experiments preceding it, especially that of the chicken, and the example, so often repeated, of animals plunged into this vapour, which are never suffocated therein suddenly, and feel no ill consequences from what they suffer'd in it, will see, that at most I expos'd myself to breathe once disagreeably; and accordingly that was all, that happened to me. Having advanced my face to the very surface of the vapour, I attempted to take in breath gently. I was sensible of something suffocating,  
much

much as when a person has his mouth near a large tube of a hot brasier, or when he goes into a very hot and moist stove. I also felt a slight acrimony in the throat and nose, which made me cough and sneeze. But this trial, which I must own was of short duration, occasioned neither sickness at stomach, nor head-ach, nor any other inconveniency. It confirmed me more than ever in the opinion, that this vapour had none of those venomous or pestilential qualities, which are attributed to *mofeta's*, tho' it is classed among them by several authors.

For my part, when I consider the quickness of its action, I see nothing in it but a fluid, the nature of which is indeed unknown to me, but which is specifically heavier than the air, and does not easily mix with it. And this I take to be sufficient to account for the effects, that are observed in the grotto.

It is well known, that the air is, for land-animals, the only proper fluid for respiration; and for this purpose it must have a certain degree of purity and density. A quadruped or a bird would soon perish for want of breath in the best and most wholesome water; and nobody could live long in a very thick smoke, tho' it were that of burnt straw, or any other more innocent matter; he would soon be smother'd in it. The same thing may be said with regard to flame; it extinguishes necessarily, when it is deprived of air; no other medium suits it. Now, of what nature soever the vapour of the grotto may be, from the moment we are certain, that it is not air, or that it is not an air like that of the atmosphere, it is easy to see, why animals cannot breathe it. They perish in it, not as poisoned, but barely are drowned in a fluid incapable of supplying the place of the air, which they

want: and it is the same with regard to the lighted flambeau.

Several reasons render this explanation plausible. First, we have seen, that the animals, which suffer'd most in the grotto, recover speedily and certainly, upon being carried into the air before they are quite dead. If the symptoms, which they have undergone, proceeded from a matter, which had injur'd some noble part, infected the mass of blood, or stopp'd the course of the fluids by some contraction or irritation excited in the solids; ought not the evil to last, in consequence of what was done, until the body were quite cleared of this matter? They no longer throw the animals into the lake, after taking them out of the grotto. It was a vulgar error of long standing, but now entirely banished, to believe, that that water was to be their antidote. It would rather give the finishing stroke to drowning them, if they were put into it, and had not strength enough to swim, and hold their head above water.

Secondly, a sort of resemblance is observed between the animals, that suffer in the grotto, and those, that are confined in an air extremely rarefied. It is well known, that reptiles and insects die with greater difficulty and more slowly in the exhausted receiver of the air-pump than quadrupeds and birds: with regard to these last especially I have frequently observed, that, when they are employ'd for the experiments of the air-pump soon after feeding, they perish in an instant, in straining to vomit. All this has a good deal of resemblance with what I have above related of the cock, frogs, lizards, beetles, flies, &c. which were confined in the vapour of the grotto.

Thirdly,

Thirdly, in fine, I have been informed by Mr. *Serrao*, secretary of the Neapolitan academy of sciences, by Father La Torre, and several other learned men of the country, that in the dissection of animals suffocated in the grotto nothing remarkable was observed, excepting that the lungs were a little too flaccid or collapsed; a state similar enough to that of an animal dead purely for want of air.

However, this testimony is not to be confounded with what the same M. *Serrao* relates of the effects of certain *mosfeta's*, which were seen for some time in the neighbourhood of Portici, after the eruption of Vesuvius in 1737. Altho' these dangerous exhalations resembled that of the grotto in many respects, yet they differ'd from it in several others: they were colder than the air of the atmosphere commonly is in summer; they turned the flesh of animals livid, that were kill'd by them; they gave a bad taste to water. Nevertheless, by attentively perusing the examination \* made of them, we find much reason to believe, that if these transitory or accidental *mosfeta's* had any bad quality more than the vapour of the grotto; it was not so much by that quality that they were either mortal or offensive to animals immersed in them, as by reducing them to an impossibility of breathing their proper element.

\* See chap. 6. of the work above-cited.



**XI. *A Letter from the Rev. Patrick Murdocke, F. R. S. concerning the mean Motion of the Moon's Apogee, to the Rev. Dr. Robert Smith, Master of Trinity College Cambridge.***

Reverend Sir,

Read Jan. 31.  
1750.

**L**AST summer, when I was to pay my respects to you at Trinity College, I gave you some account of the warm dispute, then lately arisen between Mr. de Buffon and Mr. Clairaut, two eminent academicians at Paris; the latter pretending, that the Newtonian law of attraction is inconsistent with the motion of the moon's apogee; and that its quantity ought not to be expressed by  $\frac{1}{x^2}$  of the distance, but by two, or perhaps more, terms of a series, as  $\frac{1}{x^2} + \frac{a}{x^4}$ . Which new doctrine Mr. Clairaut had got inserted in the memoirs of the academy, and Mr. de Buffon had followed him close with another memoir, confuting it.

When I first heard of this controversy, it was impossible to judge of the validity of Mr. Clairaut's reasons, because he kept his *calculus* a profound secret. But an absurd consequence of his new law of attraction occur'd to me, as soon as Mr. de Buffon mention'd the thing, that, "if we should put the attraction, express'd by his two terms, of an assumed quantity *G*, and resolve the equation, there would

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" necessarily

“ necessarily arise two different values of the distance  
 “  $x$ , for the same attractive force.”

Suspecting therefore, that some error must have slipped into Mr. Clairaut's reasonings (as he himself afterwards found there had) I resolved to try, whether, by an arithmetical calculation, from Sir Isaac Newton's propositions only, the motion in question might not be accounted for.

The result of this inquiry I should have taken the liberty to send you before now, but that, other things intervening, I did not think of revising and transcribing it, till lately; that Mr. Walmesley having made me a present of his ingenious treatise on the same subject, it appears, that, however Mr. Clairaut's hypothesis is given up, yet a notion still prevails, as if Sir Isaac Newton's propositions, concerning the motion of *apsids*, were mere mathematical fictions, not applicable to nature.

How far I have succeeded in shewing the contrary, is now submitted to your judgment. And I, at the same time, embrace, with pleasure, an opportunity of professing myself, with the highest respect,

Reverend Sir,

Stratford, 6 April,  
 1750.

Your most obliged, and

most obedient humble servant,

Pat. Murdocke.

Of the mean motion of the moon's apogee, according to Sir Isaac Newton.

The rule given by Sir Isaac Newton, in the 9 section of his first book, is to this purpose: Tab. Fig. 3.

1. That, supposing the common law of attraction, and that a central body  $T$  attracts the body  $P$ , revolving round it in an orbit nearly circular, with a force as unity; if to this be added a constant force, whose ratio to the former is expressed by  $c$ ; then the angular velocity of the body  $P$ , in an immoveable plane, will be to its angular velocity, reckoned from the *apsis* of its orbit, in the subduplicate ratio of  $1+c$  to  $1+4c$ , or as  $\sqrt{\frac{1+c}{1+4c}}$  to unity. And therefore, if  $A$  represents any arc described by the revolving body in an immoveable plane,  $A \times \sqrt{\frac{1+4c}{1+c}}$  will be the corresponding arc in its orbit, reckon'd from the *apsis*. And their difference  $A \times \sqrt{\frac{1+4c}{1+c}} - A$ , will be the *apsis* of the *apsis*.

But if the force of the central body  $T$  is diminished by some constant force as  $c$ , then the sign of  $c$  is changed in these expressions; and the direct motion of the *apsis* will be  $A \times 1 - \sqrt{\frac{1-4c}{1-c}}$ .

2. And hence, if some foreign variable force, added to, or subtracted from, the central force of attraction, produces a given motion of the *apsis*, retrograde or direct; it is easy to find a constant force as  $c$ , which should produce the same motion.

3. Let

3. Let  $S$  represent the sun, at an immense distance,  $T$  the earth (supposed, for the present, at rest)  $P$  the moon's place in her orbit  $ADBC$ , in which  $C, D$ , are the quadratures,  $A, B$ , the syzygies: then if  $PK$ , parallel to  $AB$ , and cutting  $TC$  in  $K$ , be produced till  $KL$  is double of  $PK$ ; and  $LM$  parallel to  $PT$  meet  $AB$  produced in  $M$ ;  $LM$  and  $MT$  will represent the disturbing forces of the sun, by which the moon is urged in the directions  $PT, MT$ . See *Princip.* lib. i. prop. 66. and lib. iii. prop. 25, 26.

And if  $TR$  is made perpendicular to  $LM$ , the force  $MT$  shall be resolved into two forces as  $RT$  and  $MR$ ; whereof the latter,  $MR$ , taken from  $LM$ , reduces the disturbing force, in the direction  $PT$ , to their difference  $LR$ .

4. Put now  $PT (=LM)=1$ ;  $PK$ , the sine of the arc  $PC=s$ : and then  $TM (=PL=3s):MR::1:s$ ; that is  $MR=3s^2$ , and  $LR$ , the disturbing force in the direction  $PT$ , is as  $1-3s^2$ .

When  $Cp$ , the moon's distance from the quadrature, is an arc of  $35^\circ 15' 52''$ , in which case  $1-3s^2=0$ ,  $v$  and  $r$  coincide; and the disturbing force vanishing, the line of the *apsides* becomes stationary.

But if the moon's distance from her quadrature is still greater, as at  $\pi$ , then  $\mu p$  exceeds  $\mu \lambda$ ; and their difference  $\lambda p$  is a force represented by  $-\frac{1}{1-3s^2}$ , acting in the direction  $T\pi$ . This force, at the syzygies, is double of  $TC$ .

5. Whence, and from § 1, it follows; that  $c$  being the sun's disturbing force, in the direction  $CT$ , at the quadrature; at any other point, as  $P$ , it will be  $\pm c \times \frac{1}{1-3s^2}$ . And that writing for  $c$  the variable quantity  $c \times \frac{1}{1-3s^2}$ , and  $A$  for the fluxion of the

arc  $CP$ , the fluent of  $A \times \sqrt{\frac{1+4c \times 1-3s^2}{1+c \times 1-3s^2}}$  will give

the motions of the *apfis*.

6. The quantity  $c$  being  $\frac{1000}{17872}$  of the earth's mean attractive force at the moon; by computing, as above, it will be found, that while the moon moves from  $C$  to  $p$ , through an arc of  $35^\circ 15' 52''$ , the total regrefs of the *apfis* is to the arc  $Cp$  as .005404 ( $=n$ ) to unity: and that the sum of its direct motions, while the moon moves from  $p$  to  $A$ , is to the arc  $pA$  as .0105707 ( $=N$ ) to unity.

It will be found likewise, by the inverse operation hinted in § 2, that putting  $k = .00362552$ , and  $K = .0069611$ ;  $+k$  and  $-K$  are forces, which acting constantly, the one from  $C$  to  $p$ , the other from  $p$  to  $A$ , would produce the same motions of the *apfis*.

7. The quantities  $k$  and  $K$  might have been found, pretty near the truth, only by summing the ordinates  $1 \angle R$ , or  $1-3s^2$ , upon the arc  $A$ : in which case we should have had  $k = c \times .648869 = .00370925$ , and  $K = c \times 1.24018 = .006939$ : and the motions thence computed would not have been much different from their just quantity. This however is mentioned, not as if the method itself were sufficiently exact; but to show, that if, hereafter, in cases, where the limits of the forces are incomparably narrower, we shall, instead of summing the *momenta*, make use of a mean force determined in a like manner, there is no sensible error to be apprehended.

8. Hitherto we have considered the body  $T$ , round which  $P$  revolves, as quiescent; and it is thus authors have always considered it: altho' the case in nature,  
to

to which they meant to apply Sir Isaac Newton's rule, is widely different. The earth and moon revolve about their common centre of gravity: their distances from which being inversely as their masses, and the forces, by which either is attracted by the other, as also the forces of the sun to disturb their motions, being in the same *ratio*; it follows, that the earth in her motion round the common centre of gravity will suffer disturbances every way similar to those of the moon. And the whole motion of the *apsis* of the moon's orbit, resulting from the two disturbing forces, will be near the double of what either of them could produce separately, round a fix'd centre\*.

9. To

\* “*Quatenus terra et luna circum commune gravitatis centrum revolvuntur, perturbabitur etiam motus terræ circa centrum illud a viribus consimilibus; sed summas tam virium quam motuum re-ferre licet ad lunam.*” Princip. p. 429.

And p. 141. *Apsis lunæ est duplo velocior circiter*: but this has been strangely mistaken, as if the author having revised and printed his 9th section a third time, and above forty years after it was invented, should, after all, own, that it signified nothing to his purpose. Would this be the *nil molitur inepte*, so justly applied to Newton?

See likewise, p. 423; where having deduced the motion of the *apsids* of Jupiter's satellites from that of our moon's, he adds, “*Diminui tamen debet motus augis sic inventus in ratione 5 ad 9, vel 1 ad 2, circiter, ob causam, quam hic exponere non vacat.*”

The reason is not, that the orbits of Jupiter's moons are less ex-centric than that of ours, as some have imagined; for, “*augendo vel diminuendo excentricitatem et inclinationem orbis, non mutatur motus augis sensibilibiter, nisi ubi eadem sunt minis magnæ,*” p. 180. Is it not rather, because the action of the several satellites upon their primary and upon one another, in all the possible variety of directions, reduces the case of any particular satellite to that of a single body revolving round a fix'd centre, *viz.* that of Jupiter's system?

9. To determine which, we may conceive the earth as revolving in an orbit that is already in motion from the sun's disturbing force upon the moon: the retrograde motion of the orbit, while the earth moves from  $C$  to  $p$ , being  $n \times Cp$ ; and the direct motion, for the rest of the quadrant, being  $N \times pA$ ; whence it will follow, that the disturbing force  $= k$  affects the earth's motion thro' an arc of her orbit equal to  $Cp \times \frac{1}{1+n}$ ; and the force  $= K$  acts thro' the arc  $pA \times \frac{1}{1+N}$ . And the motions of the *apsis* being in the same *ratio's*, if  $r$  is the regrefs of the *apsis* of the moon's orbit (determined as in § 6) and  $p$  its progreß; the regrefs of the *apsis* of the earth's orbit will be  $r \times \frac{1}{1+n}$ , and its direct motion,  $p \times \frac{1}{1+N}$ . That is, the whole motions of the *apsis*, resulting from the sun's action upon the earth and moon together, will be ( $R=$ )  $r \times \frac{1}{2+n}$ , and ( $P=$ )  $p \times \frac{1}{2-N}$ ; and the motions to be ascribed to either arc,  $r \times \frac{1}{1+\frac{1}{2}n}$ , and  $p \times \frac{1}{1-\frac{1}{2}N}$ .

Now  $p$ , found as above, being  $2082''.9$ . and  $N=.0105707$ ,  $P$  is  $4143''.8$ . And the same way,  $R=1375''.7$ : whose difference  $P-R$  multiplied by 4, that is,  $4 \times 2768''=11072''=3^{\circ} 4' 32''$ , is the direct motion of the *apsis* in a revolution.

#### First correction for the moon's variation. Fig. 4.

10. In the foregoing calculation, it is supposed, that the moon's orbit is nearly circular, more nearly indeed than it possibly can be, even abstracting from its excentricity. For altho' the moon had been projected with a direction and force to make her describe

scribe a circle round the earth, as  $EOL$ , the action of the sun would have changed this orbit into an oval, as  $OADBC$ ; whose greatest diameter, passing thro' the quadratures  $CD$ , is to the least as  $70\frac{1}{2}\frac{1}{4}$  to  $69\frac{1}{2}\frac{1}{4}$ . The reason and determination of which we have in *Princip.* lib. iii. prop. 26, 28.

II. That this action of the sun, and the figure resulting from it, must lessen the mean motion of the apogee, is easily shewn.

For let  $P$  be the moon's place in her orbit, when the *apsis* is stationary, and  $EOL$  the circle of her mean motion, cutting the orbit very near the octant  $O$ , and  $PT$  in  $o$ : then, the accelerating forces of the earth at  $P$  and  $o$ , being inversely as the squares of  $PT$  and  $oT$ , and the sun's disturbing force at the points  $P, o$ , being in the simple direct *ratio* of the same lines;  $oT$  being given, the *ratio* of the sun's disturbing force at the point  $P$ , to the earth's accelerating force at the same point, that is, the quantity  $c$  in the theorem, will be as the cube of the distance  $PT$ : and, *a fortiori*, in every point of the orbit, from the quadrature  $C$  to  $P$ , will exceed the mean force at  $O$ , and its effect in producing a retrograde motion of the *apsis* will be greater.

For the remaining part of the quadrant, where the motion of the *apsis* is direct, the force  $c$  is indeed greater than its mean quantity from  $P$  to  $O$ ; but, thro' the whole octant  $OA$ , it is continually decreasing as the cube of the distance from  $T$ : whence, upon the whole, that force, and its effect, from  $P$  to  $A$ , fall short of their mean quantities at  $O$ . Seeing therefore the direct motion is diminished, and the retrograde increased; their difference, that is, the direct



rect motion in the quadrant  $CPA$  will be diminished.

But this mean motion will be diminished somewhat likewise from the inequable description of the areas (in *prop.* 26. *lib.* iii.): on which account, the cubes of the distance  $PT$  must be every where increased or diminished in the duplicate *ratio* of the moments of time in which a given little angle is described, to the mean moment at the octant \*.

12. By computing from these principles, it will be found ;

1. That the angle  $CTP$ , which was of  $35^{\circ} 15' 52''$  in the circle, will, in the oval orbit, be diminished to  $34^{\circ} 43' 34''$ .

2. That the *ratio* of the mean of the cubes of the moon's distances in the arc  $CP$ , to the cube of the mean distance, will be express'd by 1.023916 (=g) and

\* To express the distance  $PT$  by  $s$  the sine of the angle  $CTP$ , in an ellipsis not very eccentric: from any point  $P$  draw  $PK$  an ordinate to the axis  $CD$ , and meeting the circumscribed circle in  $M$ ; draw likewise  $Mf$  perpendicular to  $TP$  produced. Then putting  $TC=1$ ,  $TA=d$ ,  $\frac{1-d}{d} = t$ ; by conjoining the *ratio*'s of  $TP$

to  $PK$ ,  $PK$  to  $M$ ,  $M$  to  $Pf$ , it will be  $TP = \frac{Tf}{1+ts^2}$ : in which

for the variable numerator  $Tf$ , we might, because of the smallness of the angle  $PTM$ , write unity  $1$ , but taking it rather of its mean quantity  $m$  ( $=.999987$  in the moon's orbit) the distances, whose cubes are to be summed, will be  $\frac{m}{1+ts^2}$ .

And the *ratio* of the moments of time to the mean moment is that of 110.23 to  $109.73\frac{1}{2}s^2$ , by *prop.* 26. *lib.* iii.

and the like *ratio*, in the arc  $PA$ , by  $.9852467$  ( $=b$ ).

3. Multiplying therefore the forces  $k$  and  $-K$ , found in § 6, by  $g$  and by  $b$ , substituting the products for  $c$ , in the *formula*, with the arcs  $CN$ , and  $NG$ , respectively, and finishing the operation as for the circle, the regrefs, in a periodical month, will be  $5548''.3$ , and the progress  $16489''.8$ : whose difference is the direct mean motion sought,  $3^\circ 21' 2\frac{1}{2}''$ .

13. But nearly the same conclusion may be obtained, and with much less trouble, as follows:

In the circle  $CGD$ ; take  $CM=35^\circ 15' 52''$ , and thro'  $P$ , the point where  $MK$  perpendicular to  $TC$ , cuts the orbit, draw  $TPN$  meeting the circle in  $N$ . Then, if  $R$  is the regrefs of the *apsis* in a circular orbit,  $R \times \frac{CM}{CN} \Big|_{\frac{1}{2}}$  will be the regrefs in the oval  $CPA$ .

In like manner, having inscribed in the orbit, the circle  $Amb$ , and made a similar construction for the rest of the quadrant  $P \times \frac{Am}{Ab} \Big|_{\frac{1}{2}}$ , will be the direct motion in the oval,  $P$  being the direct motion in a circle.

Thus, the angle of variation  $MTN$  being (in Dr. Halley's tables)  $33' 9''$ , the subduplicate *ratio* of  $CM$  to  $CN$  will be  $1.007927$ , and that of  $Am$  to  $Ab$ , or of  $GM$  to  $GN$ , will be  $.99499$ . And therefore  $R$  (in § 9). will be augmented to  $1386''.6$ , and  $P$  diminish'd to  $4123''$ : whose difference, multiplied by 4, gives  $3^\circ 2' 25''\frac{2}{3}$ , exceeding the former only by about  $4''$ .

14. The

14. The rule is founded in this, that if, from the centre  $T$ , a circular arc  $Ff$  be described, including in the angle  $CTN$  the sector  $FTf$ , equal to the elliptic sector  $CTP$ , the cube of  $TF$ , the radius of this circle, may be taken for the mean of the cubes of the moon's distances in the arc  $CP$ . And because the area  $CPT$  is to the sector  $CMT$ , as  $PK$  to  $KM$ , or as  $TA$  to  $TC$ ; and  $To$  or  $TE$  is a geometrical mean between  $TA$  and  $TC$ , it will easily appear, that  $TF^3 : To^3 :: CM^{\frac{3}{2}} : CN^{\frac{3}{2}}$ . And that  $P$ , found from the tables, being (nearly at least) the stationary point in the oval, if the force  $k$  is increased in the sesquiplimate ratio of  $CM$  to  $CN$ , and the arc  $CN$  substituted for  $A$  in the formula, we shall, by § 1, find the retrograde motion of the *apsis*.

Now, when the constant force  $+k$  is given, the regrefs  $R$  is as the arc  $A$ ; and when  $A$  is given, and  $k$  is but a little augmented,  $R$  is proportional to  $k$ : in general therefore, if  $k$  is but a little augmented,  $R$  is as  $k \times A$ . Write  $Q$  for the regrefs in the oval,  $R$  standing for that in the circle, already found; and it will be  $Q : R :: k \times \frac{CM}{CN}^{\frac{3}{2}} \times CN : k \times CM$ , or  $Q = R \times \frac{CM}{CN}^{\frac{1}{2}}$ , according to the rule. The like reasoning for the direct motion,

Second correction for the Excentricity. Fig. 5.

15. This equation is inconsiderable; because, altho' the ratio of the disturbing force, when the moon is at a greater than her mean distance, is more increased than it is diminished in the opposite points of her orbit;

orbit; this increase is very near compensated by the comparative smallness of the angular velocity.

Let  $ADa$  represent the moon's elliptic orbit, whose centre is  $C$ , its axes  $Aa$ ;  $Dd$ , the mean excentricity  $CT$ , and the circle of her mean motion  $MDmd$ , cutting  $Aa$  in  $M$  and  $m$ . Then, because it is a mean motion we seek, generated while the axis  $Aa$  passes thro' all its different aspects of the sun; we may conceive the direct motion already found, of  $3^{\circ} 2' 21\frac{1}{2}''$ , to be produced by a constant disturbing force —  $K$ , acting on the moon as she revolves in her circular orbit  $MDmd$ ; and we have only to enquire, how much this force, and its effects, are to be increased, the moon really moving about the same centre  $T$ , in the elliptic arc  $AD$ ; and how much diminished in the arc  $Da$ .

16. For which purpose, the constant force  $K$  is to be increased in the *ratio* of the mean of the cubes of the moon's distances, in the arc  $AD$ , to the cube of  $TD$  or  $CA$ , and diminished as the mean of the cubes of the distances in  $Da$ . Let the forces resulting be  $\dot{K} \times G$  and  $\dot{K} \times H$ ; and these being substituted in the *formula*, with the arcs  $2DM$ ,  $2Dm$ , respectively, the sum of the motions found will be the whole mean motion of the apogee, including the correction for the excentricity.

Now  $K$  will be found to be .00557337, and the excentricity  $TC$  being .05505, and  $Q$  the quadrantal arc to *radius* 1; the *ratio*  $G$ , or, which is the same, the sesquiquiplicate of the time, in which the elliptic arc  $AD$  is described, to the time in the circular arc  $DM$ , that is,

$$\frac{Q \sqrt{C}}{DM} \Big|_2^3 \text{ will be } 1.110942; \text{ and } H \left( = \frac{Q - TC}{DM} \Big|_2^3 \right) =$$

=.9001387: whence the whole motion, found as above directed, will be  $10962'' = 3^{\circ} 2' 42''$ ; the correction, on account of the excentricity, being only  $21''$ .

Multiply  $3^{\circ} 2' 42''$  by 1.080853, and the product  $3^{\circ} 17' 28''$  is the mean motion of the apogee, in a synodical month; exceeding the quantity marked in the tables by no more than  $4''$ .

17. Of the obliquity of the moon's orbit, to the plane of the ecliptic, we take no notice: because, altho', absolutely speaking, a force in that plane, referred to the moon's orbit, would, thence, be diminish'd by about  $\frac{3}{1000}$  parts; yet, in the present case, the effect of the obliquity is included in the first determination of the quantity  $c$ , from the periodical times of the earth and moon; all but what belongs to the corrections; and which is only  $110''.0003 = 0''.33$ , to be subtracted.

18. The force  $c$  is, itself, the effect of the sun's parallax, and the total effect; excepting only a small difference between his action on the moon, when she is waxing or waning, and when she is in the other half of her orbit; neglected as altogether inconsiderable.

Upon the whole, we may conclude, that, in this, as in the other phænomena of the celestial motions, the principles and rules of Sir Isaac Newton are fully confirmed and verified.

*XII. Experiments made on a great Number of living Animals, with the Poison of Lamas, and of Ticunas, by Monsr. Herissant, Doctor of Physic, and F. R. S. Translated from the French, by Tho. Stack, M. D.*

Read Jan. 31. 1750. **M**ONSIEUR de la Condamine, of the royal academy of sciences of Paris, on his return from the voyage, which he made in the inward parts of South America from the coast of the South Sea to the coasts of Brasil and Guiana, by going down the river of the Amazons, brought to Paris a small quantity of a very dangerous poison, much in use among the Indians of Lamas \*, Ticunas, Pevas, and also among the Yameos, who all extract it by fire from divers plants, especially from certain plants, which the French call Lianes.

Those savages are very dextrous at making long trunks, which are the most common weapon used by

K 2

the

\* Lamas is a Spanish village, or little town, in upper Peru, situated in about seven degrees of south latitude to the west of the river of Guallaga. The native Indians of this district prepare a famous poison for poisoned arrows, different from that of the Yameos, Pevas, and Ticunas, Indian nations, on the borders of the river of the Amazons, towards the mouth of the Napo, in three or four degrees of south latitude.

The poison of Ticunas is the most famous of all for its activity. They say, that that of Lamas sooner loses its force, but that it is properer for certain animals than that of Ticunas. And it is the common opinion, that that of Lamas being mixed with that of Ticunas becomes more violent and active by the mixture.

the Indians for hunting. To them they fit little arrows made of palm-tree, on which they put a little roll of cotton, that exactly fills the bore of the tube. They shoot them with their breath, and seldom or never miss the mark. This simple instrument advantageously supplies the defect of fire-arms among all those nations. They dip the points of these little arrows, as well as of those of their bows, in this poison; which is so active, that, in less than a minute, especially when fresh, it kills certain animals, from which the arrow has drawn blood.

Monsieur de la Condamine says, in the abridged relation of his voyage, that “when he arrived at Cayenne, “he had the curiosity to try, whether this poison, “which he had kept above a year, still retained its activity; and, at the same time, whether sugar was “really as efficacious a counter-poison as he had been “assured. Both the experiments were performed, says “he, in presence of the commandant of the colony, “of several officers of the garrison, and of the king’s “physician.

“A hen, slightly wounded with one of these little arrows, the point of which had been dipp’d in “the poison thirteen months, at least, before the “trial, blown thro’ a trunk, liv’d half a quarter of “an hour: another, prick’d in the wing with one “of these arrows, newly dipp’d in this poison diluted “with water, and immediately drawn out of the “wound, seemed to doze a minute after; convulsions soon came on, and, tho’ we had made “her swallow some sugar, she expired. A third, “prick’d with the same arrow, dipp’d again into “the poison, having been instantly assisted by the “same

“ same remedy, shew’d no signs of being indisposed,  
 “ &c.”

I was struck with amazement on reading these facts : but my surprize was soon follow’d by a desire of repeating those experiments myself, and even of trying them on different sorts of animals.

Monsieur de la Condamine, to whom I imparted my intention, offered, with the best grace in the world, to satisfy my curiosity, and for that purpose made me a present of a certain quantity of this poison : and the result of the experiments, which I made with this same poison, will be the subject of this memoir.

I will begin the detail of those experiments by that of two accidents, which had like to have disabled me from prosecuting the work I had undertaken ; having very narrowly escaped death.

The first accident happen’d thus : M. de la Condamine had forewarned me, that, when the Indians designed to use their poison (which, in colour, consistence, and even in smell, has a great deal of resemblance with Spanish liquorice) they dissolved it in water, and then evaporated it on a slow fire to the consistence of a soft extract. I made this preliminary preparation in a small closet, in which a young lad was actually at work ; and I did not think of making him quit it, because I did not imagine, that the poison, of which I intended to make trial, could produce any bad effects, without being introduced into the blood, by the opening of a wound. Nor did I then recollect what M. de la Condamine had told me ; which is, that, while they are preparing this poison in the country, they oblige some criminal old woman to take care of the boiling of  
 this



this poison, after shutting her up alone in a separate place: so that, when this woman dies, 'tis a sign, that the poison is sufficiently boil'd, and that it has all the qualities requisite to make it good. But I was soon made sensible of my imprudence: the door of the closet, where the young lad above-mention'd staid, was open; and from the next chamber I saw, that the lad, who had been there about three quarters of an hour, sat still, with his arms across. I began to reprimand him for his laziness; but he excused himself, by answering, with a trembling voice, that he was sick at heart, and felt himself very faint. 'Tis easy to imagine the uneasiness, which this sight gave me; but luckily it cost me no more than the fright. I made the lad come out of the closet immediately, led him down into the yard, and made him swallow a pint of good wine, in which I had dissolved a quartern of sugar. He recover'd his strength by degrees, and was soon able to return to his own home, very merry and happy, without the least notion of the danger he had been in. Some days afterwards he came to me, and assured me, that he had not felt the least indisposition since the day in question.

The fact above related was shocking enough to make me abandon my project: however, curiosity got the better of my fear; and I even took a strong fancy to repeat the experiment. It would be inhuman, not to say criminal, to make it on any other person but myself: wherefore I resolv'd to run the risk, or rather, I persuaded myself, that I should run none, because I should be timely enough to flee from the danger, as soon as the effect of the poison should come to a certain pitch. Besides, I was encouraged  
by

by the good success of the foregoing example. Therefore I disposed of every thing as at the first time, and I staid in the closet. In about an hour's time I perceived my legs to bend under me, and my arms became so weak, that I could scarcely use them. I had but just time enough to come quickly out of the closet, and get down into the yard; where I order'd wine and sugar to be brought me, as I had before done for the young lad.

Such was the first danger, which I incurred in preparing the American poison: the second was not inferior to it.

After having dissolved the poison of Ticunas in water, and reduced it to the consistence of an extract in the manner above described, I put it into a phial, which I stopped very exactly, and locked up in a desk, till I should have occasion to use it in the experiments, which I intended to make. I began these experiments on the 6 of June 1748; which was so hot a day, that I stripped to my shirt, and had my breast and arms exposed to the air. In my left hand I held the phial, the cork of which flew up to the ceiling with vast rapidity. At the same instant there issued out of this phial a yellowish vapour, of a very penetrating smell, which was soon followed by the extract itself, that spread itself all over the rim of the neck of the bottle. I was so stupified at this unexpected accident, that I imagin'd (as it was very possible) that the bottle was broken in pieces: and as soon as I saw my hands, arms, and breast, colour'd in several places by the poison, which had besprinkled them in the explosion, I look'd on myself as a dead man: which must certainly have been the case, if

the bottle had burst, and the pieces of glass had scratched or cut me. But luckily that did not happen; and I soon resumed courage; when, after some minutes, I found myself quite as well as before the explosion of the poison, the effect of which is almost instantaneous; and it gave me no other trouble than to wash and dry myself very carefully.

From this accident I learned, that this poison, thus prepared, ought not to be put into glass bottles close stopped, but should rather be kept in a glazed earthen pot, covered with paper only; since it was susceptible of so great an effervescence. Wherefore I put it into a gallypot; and the experiments, which I made with this same poison a good while afterward, convinced me, that there is no reason to apprehend, that it would lose any of its activity by evaporation.

These two facts plainly shew, how much precaution ought to be taken, when this poison is to be used. And we shall be the better convinced of it, when we consider, that one single drop, conveyed directly into the blood by a puncture, &c. is sometimes sufficient to kill, or at least to cause great disturbance in the animal œconomy. It is quite otherwise, when taken in at the mouth; for then it does no sort of mischief, as I shall prove in another place.

Let us now pass to the experiments, which I have repeated a number of times on different species of quadrupeds, birds, fishes, insects, and reptiles. But I must first observe, that, of all those animals, none but quadrupeds and birds were killed by this poison, as will more particularly appear by the journal  
of

of my experiments: the others, *viz.* the fishes (*a*), the insects (*b*), and the reptiles (*c*), were not killed, tho' several of them seem'd to be disorder'd by it.

I have verified what M. de la Condamine says, in the account of his voyage, relating to the use, that may be made of animals killed by this poison, without apprehending any ill consequences to those, who eat of them. In effect, I have eat rabbits, which I had killed with this poison, and afterwards made several other persons eat of them; and not one of us perceived the least indisposition.

*Journal of the Experiments.*

On the 6 of June 1748, I made a little wound, of about three lines long, in the left hinder leg of a rabbit of six months old: into this wound I put a bit of cotton soaked in the poison of Ticunas: the creature died suddenly in my hands, without giving the least indication of having felt pain, and even before I could apply a bandage to the wound.

The same day I repeated this experiment on eight other rabbits, and on four dogs: they all died in a minute, or thereabout.

The seventh of June of the same year I dipp'd the point of a lancet into the poison: and with this instrument I prick'd four cats and two rabbits, some  
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(*a*) Those, which I employed, were the carp, the eel, the pike, the gudgeon, the barbel, and the tench.

(*b*) As caterpillars, bees, different flies of two and four wings, the grillo-talpa, butterflies, may-flies.

(*c*) For example, earthworms, vipers, snakes.

in the head, and the others in the paw. The rabbits died in as short time as the preceding day ; but the cats held out about three minutes. It is to be observed, that each time, that I prick'd an animal, I took care to make a new dip of the lancet into the poison.

The same day I made a little wound about two lines long in the right hinder leg of a rabbit, and put into it a small pledget of cotton soaked in the extract of opium diluted in a little spirit of wine : but this did not cause any disorder in the creature ; nor did arsenic, which I applied to another in the same manner. In fine, to a third I made use of the extract of white hellebore, and I perceived, that this animal became restless, nearly as I had observed in the animals, that died by the effect of the poison of Ticunas. However, this rabbit did not die, but fell into a sudden fit of fury, which went off in about eight minutes. I have likewise made trial of this extract on other rabbits, dogs, and cats ; and the effect was the same, more or less. Of all the extracts, which I employed, as, for example, those of henbane, nightshade, tobacco, &c. I found none but that of white hellebore, that seem'd to raise some little disorder in the animal oeconomy. The essential oil of the ~~*Lamp-cerasus*~~ did not incommode the animals, into whose mass of blood I conveyed it, instead of the poison.

The eighth of June, with a lancet I made a very small incision between the ears of a cat, and with a pencil I put into it a drop of the poison of Ticunas mixt with that of Lamas : in an instant the creature died between my hands.

June the ninth, I put some of the same poison into small wounds, which I made in different parts of insects, reptiles, and fishes; and not one of them died of it.

The same day I made a wound, that penetrated into the cavity of the abdomen of a large cat, without hurting any of the contained parts; and, with a crotchet, holding up the integuments, to keep them from touching the abdominal *viscera* of this animal, that lay on its back, I introduced the end of a funnel, and thro' it poured into the cavity of the abdomen about half a drachm of the poison of Lamas mixt with that of Ticunas. By this management I intended, that the edges of the wound should not be wetted with the poison, and that it should touch nothing but the surface of the abdominal *viscera*. I made a suture of one stitch to join the lips of the wound, and I kept the integuments constantly suspended, to prevent their touching the poison: and in this I am certain that I succeeded. At first the creature did not seem to suffer much from this operation; but in an hour's time he died, with such violent convulsions in his throat, that it was almost impossible for him to breathe.

June the tenth, I prick'd with a lancet the left fore leg of a large fat cat, and put in a drop of the poison of the Ticunas. I let this animal run loose about the room, without dressing the wound. By the time he had made a turn round the room, he seem'd very restless and timorous: his legs fail'd him; he lay flat on his belly; and I remarked, that the skin all over his body trembled considerably; the hair of his tail stood up, and his paws were agitated with

a frightful tremor. All this while the animal made no noise: in fine, his head fell all at once between his fore legs, and he died in four minutes after the insertion of the poison.

June the twelfth, I made the same experiment on two other cats, and on three dogs: these animals seem'd to fall sick almost in an instant: the cats had their hair bristled up, and their bodies gather'd into a heap: they scratched the ground with their fore-feet. The dogs did the same, and all of them had a languishing look, and their eyes bathed in tears some of them looked at me stedfastly, and made a mournful noise: they were seized with a shivering, and, in fine, they became paralytic in their feet only; after which they died, turning their head very quick to the right and left, with their mouth wide open. During this scene, I perceived a spasmodic contraction in all the muscular parts of the neck.

The fifteenth of July I pricked a hawk in the left claw: into the puncture I introduced a small drop of the poison of Ticunas mixt with that of Lamas, and then set the creature at liberty. From that moment it was impossible for him to fly; the most he could do was to perch on a stick, which was within six inches of the ground. There he shook his head several times, as if to get rid of something that seem'd troublesome in his throat. His eyes were restless, and his feathers were all bristled up. In fine, after several gapings, his head fell all at once between his legs, and he died thus with his wings expanded. The time he spent in dying was three minutes from the insertion of the poison. I repeated this experiment

on several sorts of birds (a), and they all died with pretty much the same symptoms as those above-mentioned, and in as short a space of time. I made six of these birds swallow a good dose of sugar, before inoculating them with the poison: three of them escaped death, but the other three died very soon. Moreover, the moment after inserting the poison into four other birds, I made them swallow a good deal of sugar; but that did not prevent their dying, almost as soon as those, that had taken none. I made other birds swallow sea-salt instead of sugar; and not one of them recovered, whether they took it before or after the application of the poison.

July the 16, I put a little of the same poison into a small wound, which I had made in the right fore-foot of a young rabbit. The moment this operation was performed, I cut off that foot above the place of insertion of the poison. I dressed the stump, and the animal did not die. Some days afterwards, I repeated this experiment on two large dogs, and on a lamb; and not one of them died.

July the 20, I made a tight ligature on the right hinder leg of a young rabbit, in order to see, if I could thereby prevent the poison from penetrating too quick into the mass of blood. That done, I put a drop of the poison of Ticunas and Lamas into a small wound, which I made below the ligature: but this notwithstanding, the animal died in less than two minutes.

July

(a) As pigeons, hens, blackbirds, sparrows, ducks, geese, and magpies.



July the 22, I poisoned the point of a sword with the same poison ; and with this sword I pierced the left thigh of a large cat, which died in a minute, without shewing any signs of suffering.

July the 24, after having introduced some of the same poison into little wounds, made in the legs, and other parts, of several dogs, cats, foxes, and horses, I immediately applied a red-hot iron, or burning charcoal, on the wounds : not one of these animals died : but this operation must be performed very speedily.

July the 30, I pricked a great number of rats and mice in the feet with a lancet, after poisoning its point. They all died in less than a minute, after being tormented with a frightful shivering, which was immediately followed by an almost general palsy. The same thing happened to moles, which I made use of for this experiment.

August the 6, I made a small wound in the left hinder leg of a pig of three months old ; and then I put into it two drops of the poison of Ticunas : this creature died in six minutes. I repeated this experiment on two young wolves, which died in the same space of time.

August the 7, I cut off the tip of the ear of six puppies, and rubbed the part with the poison of Ticunas : not one of these animals died of this operation. Two days after, I shaved the hair off of their backs very close, and rubbed the part with the same poison : they all died in less than three minutes.

The 10, 11, and 12 of the same month, into small wounds made in different parts of the body of several dogs, cats, polecats, guinea-pigs, &c. I instill'd seven or eight drops of blood, which I drew from

from the *vena cava* of a dog, which I had killed with the poison of Ticunas mixed with that of Lamas. These animals did not die indeed, but were plainly indisposed; inasmuch as they lost their vivacity, and became very fullen. Eight days after this experiment, I repeated it on these same animals; and then they became still weaker and fainter. In fine, the next day I made it a third time on them, when they languished four or five days, and then died.

August the 15, after having put some of the same poison into a wound made in the right hinder leg of six horses, one of which was a very vigorous stone-horse, I quickly bled them all in the neck *ad animi deliquium*: two of them escaped with life; but those, that were the weakest and most worn out, could not stand against this operation. Two days afterwards, I again pricked those horses, that did not die of the last experiment; and then they died in about eight minutes.

I made the following observations on these animals, from the insertion of the poison to their death. The muscle, wounded by the incision made for insinuating the poison, was contracted and relaxed alternatively, just as it happens in animals fresh killed: this lasted about two minutes; after which these animals seem'd restless and impatient, endeavouring to scrape the ground with their fore-foot, which I had suspended in the air with a cord, to prevent their running away. Sometimes also they made a sudden effort, as if to get away, which lasted the space of two minutes; after which they grew quiet, and amused themselves with nipping the grass, but not in a natural manner. Then their respiration became  
very

very difficult; and, tho' the weather was very hot, there visibly came out of their nostrils a vapour, like that which issues in winter in the time of expiration. A minute after, I observed, that these horses endeavour'd to rest the suspended leg on something: and, in another minute, I perceived the fore-leg, that rested on the ground, beginning to grow weak, and bend; which occasion'd these animals, to fall forward, and rise up again, alternately, with more or less difficulty. In two minutes more, their hind-legs grew weak, and bent under them, like the fore-legs; and, in fine, these animals fell down like a dead lump, without being able to rise again, tho' I whipp'd them heartily. Then their sides began to work, and the whole habit of the body was seized with a dreadful horror. I whipp'd them, and prick'd them with a pin; but in vain; for they gave no sign of feeling. All the muscles of the trunk and extremities were become paralytic; and none retained their action, but those of respiration, and those of the ears and eyes. These creatures continued in this condition about two minutes; after which I observed, that their respiration grew so operose, that each inspiration consisted of three successive attempts, and then followed a most precipitate expiration, accompanied with so violent an hiccup, that, the body bending double, the hind-legs were pulled quite to the fore-legs. In fine, this manner of taking in and letting out breath lasted one minute; in which time their eyes were darkened, and death ensued.

I opened the dead bodies of these horses, and observed as follows;

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The blood was of a deep brown colour, and spouted out in a full stream, which lasted near a minute, both from the arteries and veins, which I cut. This phænomenon surprized me much, as well as the horse-slayer, who attended me, and assured me that he had never seen the like. The muscles were flaccid, blackish, and very cold. The heart was so violently contracted, that, in cutting it across, I could not see any appearance of the ventricles, until I pull'd their sides asunder by force. The lungs and liver were stuffed with blood.

In making the small wounds, for introducing the poison, great care must be taken, to avoid cutting any trunk of any artery or vein; because, when that happens, the blood, that issues out, carries off a good part of the poison; which makes the animal pine more or less without dying; or, if he dies, it is in a longer or shorter time, according to the quantity of the poison, that has got into the vessels, and been mix'd with the circulating fluid. This thing happen'd to me at M. de Reaumur's house, in trying the experiment on one of his mares, which had been condemned to the laystall. This beast lived above four hours, because the wound bled abundantly, and hinder'd the success of my experiment, for the reasons alleged above.

On the 18 of November I took a little steel arrow, of the following shape and size,



and poisoned it with the poison of Ticunas mix'd  
M with

with that of Lamas. I caused this arrow to be shot into the right hinder leg of a bear, belonging to M. de Reaumur, which he wanted to have killed, in order to put it into his cabinet of natural history. The creature immediately roared out, from the anguish of the puncture ; after which he made a tour round the stable, in which he was, without seeming to be in any pain. Soon afterwards he fell on his side, and died in less than five minutes, having his throat squeezed, as if he had been strangled.

M. le Chevalier de Grossée had an eagle, which he had kept a good while in his court-yard, and intended to make a present of it to M. de Reaumur, to adorn his cabinet, but wanted to know, how to put it to death without damaging the feathers. M. de Reaumur sent him the same arrow above-described, which I had fresh-dipp'd in the poison ; it was struck into the wing of this large bird, which dropp'd down dead in an instant.

Such are the chief experiments, which I made with the poison of Ticunas and Lamas : and here follows the result of my observations.

1. In almost all the animals, which I killed with the poison of Ticunas and Lamas, I observed, that, in general, they seemed to feel little or no pain before dying, by the action of this poison.

2. That, before they die, these animals are seized with a sudden and almost universal palsy.

3. Tho' the colour of the blood seemed to me to be altered in certain animals, yet we ought not to draw any inference from thence ; because, in many others, the blood had undergone no sort of alteration, either in colour or consistence.

4. That all the muscles are so vastly contracted in the animals thus poisoned, that there is not a drop of blood to be found in them, whatever way you cut into them. These muscles are clammy to the touch, and seem to approach the condition of flesh beginning to be tainted, which feels clammy.

5. That I do not know a more certain rule for determining, that an animal died by the energy of this poison, than this state of the flesh, which feels clammy immediately after death: but a person must have handled it more than once, if he would avoid being mistaken.

6. That the whole mass of blood, during the action of the poison, is carried in abundance into the liver and lungs.

7. That neither sugar nor sea-salt ought to be regarded as a specific antidote; because the poison operates so quick, that it does not allow time to these drugs to act, so as to prevent death. I have found nothing but red-hot iron applied in time, that cures with sufficient certainty.

8. That the more the animal is of a lively and sanguine constitution, the more speedily and forcibly the poison acts.

9. The lustier and fatter the animal is, the more poison and time also are required for producing the expected effects.

Before I make an end, it is worthy of observation, that the poison must be dried on the instrument, before it be struck into the animal, which we intend to kill: for, if it be liquid, it remains on the outside of the wound, while the instrument penetrates into

the flesh: in which case, either the animal dies not at all, or at least with great difficulty: as it happen'd to me at M. de Reaumur's house, with regard to a young wolf, which did not die, tho' the arrow above-mentioned was stuck into one of his thighs; because the poison, which it retained from the dip, continued liquid, and remained on the outside of the wound made by the arrow in piercing the flesh. Wherefore time must be allowed to the poison to grow hard on the instrument, which is intended to be used; that so, entering into the wound together with the weapon, it may be there diluted, and carried in the course of the circulation to those parts which it must affect, in order to cause death.

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*XIII. The Case of a Woman, from whom the Bones of a Foetus were extracted. By Mr. Thomas Debenham, Surgeon, at Debenham in Suffolk. Communicated by the Rev. Mr. J. Clubb, Vicar of that Parish, to William Battie M. D. F. R. S.*

Read Febr. 7.  
1750.

ON the 25 of April 1749, the wife of one Benjamin Last, a cooper, in the parish of Debenham, in the county of Suffolk, aged about 34 years, being pregnant of her eighth child, had all the symptoms of a woman in labour.

labour. Accordingly, a midwife was sent for ; who, from the violence of the pains, expected, that she would soon be delivered ; but, to her great surprise, nothing ensued but a loss of blood, and the pains were considerably abated. A fever immediately came on, which cast her into an excessive faintness, and loss of strength, accompanied with a nausea.

On the 26 of May, I was desired by her husband to visit her ; and, by the account she gave me, I much suspected, that she must have miscalculated with regard to her time ; and I proposed to examine her : but she, out of a mistaken modesty, not complying, I contented myself with cooling injections, mild cathartics, and cordial powders, &c. ; by the use of which medicines she grew better ; and, on the 26 of March following, undertook to walk a journey of 15 miles.

I heard no more of her for the present ; but, on the 27 of April 1750, the pains returned, very much like those of labour ; which obliged her husband to call me out of bed. I immediately gave her an anodyne, which abated her pains, and composed her to rest.

On the 14 of May, she felt a pricking pain in her navel, with a swelling and redness, which, in a few days, appear'd like a boil ; when, being desired to inspect the tumour, I applied an emollient cataplasm. The next morning, upon removing my dressings, a fetid matter ensued ; whereupon, dilating the small *sinus* with my scissars, the *scapula* of a *fœtus* presented itself. On the 25 of July, by the direction  
of



of a physician, I undertook, by making a circular incision round the navel, to enlarge the orifice into the cavity of the *abdomen*, in order to extract the *fœtus* that way: but the woman being very weak, and much emaciated, I could now only take off the *scapula*.

The next day, I extracted one whole arm, some ribs, part of the *vertebræ*, &c. and, the day following, the greatest part of the remaining *fœtus*, except the *cranium*, which seem'd to adhere to the intestines. This determin'd me to proceed very cautiously, and not to attempt the removal of it at once, but piece-meal, and by degrees, as opportunity would give me leave; which I did with my forceps: but, notwithstanding all my care, the sharp edges of the broken pieces of the *cranium* tore the intestines, so that the *fæces* issued from the wound at every dressing for several weeks together.

The wound was daily dress'd with dry lint, spirituous fomentations, and cataplasms. Injections, made of sack and warm water, were found of great use, thrown in in large quantities; and (what is well worth observation) several parts of the bones, as the *tibia*, *fibula*, &c. were discharg'd by the *vagina*.

By the means above-mention'd, and proper bandages, the wound was thoroughly deterged, incarned, and, by the use of epulotics, completely cicatrized; and the woman is now perfectly recovered, and since grown fat."

*N. B.* After the discharge of the whole *fœtus*, the patient had milk in her breasts, as upon a natural delivery.

Debenham, Jan. 18.  
1750-51.

Tho. Debenham.

XIV. *New Discoveries relating to the History of Coral, by Dr. Vitaliano Donati. Translated from the French, by Tho. Stack, M. D. F. R. S.*

Read Feb. 7. § 1 **C**ORAL is known to be a marine vegetation, which in shape nearly resembles a shrub stripped of its leaves.

§ 2. It has no roots, but is supported on a broad foot, or basis, which adapts itself, as wax well-press'd, and sticks to any body in all its parts, with such firmness, that it is utterly impossible to disengage it. The shape of this foot is not always the same; but, for the most part, it approaches to roundness (Tab. III. Fig. 1. *n n*). The only use of this part is to hold the coral fixed, and support it; not to nourish it: since there are found pieces of coral, with their feet broken off, and separated from the place that supported them; which pieces nevertheless continue to live, to grow, and to propagate, at the bottom of the sea.

§ 3. From this foot arises a trunk, generally single, the greatest thickness of which seldom exceeds an inch Paris measure, as I have been assured by old coral-fishers.

§ 4. Out of this trunk the branches shoot, which commonly are few in number ; and they afterwards divide into several smaller and slenderer branches. For the most part, the branches are disjoined, and stand separate ; but yet it is sometimes observed, that two or more branches spring from the foot united and parallel, and, as it were, clung together so intimately, that the place of their union cannot be distinguished. We frequently see two branches adhere and unite in the same manner, in whatever place they happen to touch : and I have likewise observed, that, from two branches thus united, there arose afterwards but a single branch.

§ 5. One thing seems to me worthy of notice ; which is, that, if a shell happens to stick to the trunk or branches of the coral, it is in time surrounded and covered, either in part, or in the whole, with the same coralline matter, to which it stuck.

§ 6. The greatest height, to which I have seen coral rise in the Adriatic, is a Paris foot, or some little matter more. And even this height is very rare in that sea.

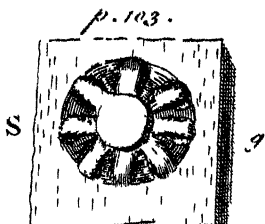
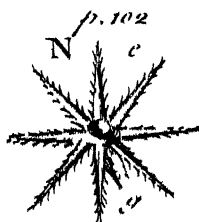
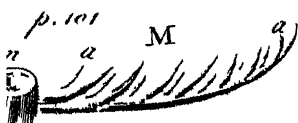
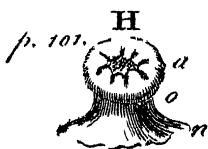
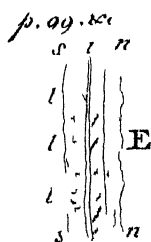
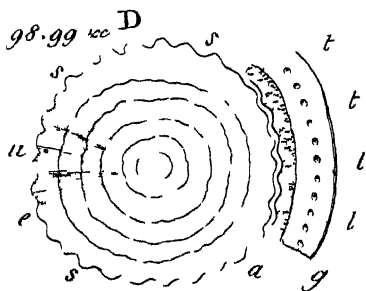
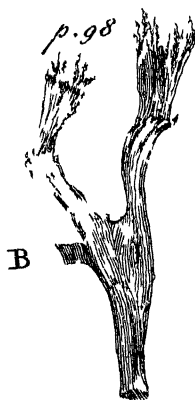
§ 7. The trunk, as well as the branches, are commonly round ; and yet we frequently find, that some are flatted and broad, of which I have some specimens in my collection.

§ 8. The foot, trunk, and branches of this sea-production are of one uniform matter ; that is, they are formed of a substance homogeneous in all its parts, and of a bark or coat.

§ 9. The substance forms the inward part of the coral ; and this, even at the bottom of the sea, is of an hardness little inferior to that of marble. At the  
ends









ends of the branches it is not so hard as the bark; in some places near the ends it is of equal hardness with it; but in the thick branches and trunk it is harder.

§ 10. This substance, being observed by a microscope, in corals of one colour, as the red, and those which are not corroded by worms, appears uniform, smooth, without spots of other colours, without holes or pits, but quite even, hard, and capable of a perfect polish.

§ 11. But it is otherwise, in corals of more colours than one; as, sometimes, in those of a yellowish rose-colour, and those of a rose-colour. For I have some branches of these, the transverse sections of which exhibit different lines, or annular bands (Fig. *D. s, s, s, s,*) whereof one part is a rose-colour, and the other yellowish, others white, and others more or less charged with colour, which form concentric circles, *D. a,* like the coats of an onion.

§ 12. The same sort of annular lines is observable in red coral a little burnt, but they are of a grey colour, and parted asunder by a line of a deep-brown grey (*s, s, s,* Fig. *D.*).

§ 13. When this substance, tho' very hard, happens to be stripped of its bark, either by age, or some other accident, it is liable to a sort of *teredo*, or worm; which is a small animal, that enters into the body of the coral by very small holes. (Fig. *C. a, a,*) gnaws its inside, and makes itself roundish cells therein, (*C. s, s,*). These cells have a communication with each other, (*C. a, a,*) and are separated by very thin partitions, which weakens the coral extremely, and makes it brittle and improper for any sort of work.



§ 14. There is also another worm, which passes thro' the coral transversely from side to side, and in right lines, by strait cylindrical holes.

It may not be improper to take notice here, that the hardest marbles, lying in the sea, are liable to be corroded in the same manner.

§ 15. The surface of the substance of coral is furrowed and wrinkled (Fig. B. Fig. D. *e*, *u*). The wrinkles begin from the foot, and ascend, always nearly parallel, to the trunk and branches. However, these wrinkles are not so deep in the slender branches, and sometimes are not visible there: but they are always more elevated, and more considerable, in the thick branches and trunk: they are not smooth, but uneven, with knobs or bumps on them, and the surface, as it were, composed of very little hemispheres.

§ 16. This substance of the coral, being exposed to a strong fire, is reduced to a very fine ash-colour'd powder. As common ashes, when taken clean from burning charcoal, and examined by a microscope, exhibit a sort of skeleton, composed of the fibres and vessels of the wood; so the ashes of the substance of coral sufficiently point out, of what sort of parts it is composed. The microscope discovers therein ashes, formed of very small white corpuscles, united in clusters; each one of which is nearly spherical. The ashes of the bark of the coral are of the same shape and colour; so that the substance of coral agrees with its bark in the primitive and constituent parts (if I may be allow'd the expression) which seem to be the same in both.

§ 17. In pieces of coral broken transversely, I have often observed some prominent wrinkles, which,  
disengaging

disengaging themselves from the exterior wrinkles above-mentioned, ran towards the center (Fig. *D. u.*). Hence it plainly appeared, that there is an affinity or connexion between the interior and exterior wrinkles.

§ 18. To the exterior wrinkles, and to the whole outward surface of the hard part of the coral (Fig. *D. a, s, e, s.*) there is closely attached a white or pale pellicle (Fig. *D. g, E. n, n.*) which is pretty soft, and composed of valcular and follicular minute membranes, which, by their interlacing, form a reticular body. The whole is accompanied with small vessels, which contain a whitish juice, that is diffused thro' all the *folliculi* or *membranulæ*; which have also attached to them certain very small red corpuscles, united together by means of other *membranulæ*.

§ 19. These corpuscles are nearly of a spherical figure, and, in size and shape exactly like those of the ashes of the coralline substance, and of the bark: so that we may properly say, that these little bodies constantly remain intire, even after the action of the fire; having undergone no other change but in their colour.

§ 20. In this pellicle (*E. n, n.*) the globular corpuscles are not numerous, but the greatest part of the said pellicle is occupied by very white membranes, from which it takes its colour, and not from the red globular corpuscles.

§ 21. This pellicle, lying immediately on the coral, deposits the red corpuscles, and adapts them to it: and thence it is, that the wrinkles are cover'd, as it were, by extremely little hemispheres; and these infallibly shew the formation of the coralline substance. If any one should ask, whence can these

little spheres derive their origin? my answer would be, without hesitation, from the *polypi* of the coral. And the reason is, that, if these *polypi* produce their eggs, as will be shewn in the sequel, covered with such corpuscles, we may justly infer, that corpuscles of the same nature, where-ever they are found, are formed by the same *polypi*.

§ 22. To this white pellicle is attached the bark of the coral (Fig. *D. t, t, E, s, s,*) which is soft, of a vermillion-colour, or of a brighter colour than the coralline substance. It is formed of very fine *membranulæ*, or net-work; to which are annexed, and reciprocally fastened, the red globular corpuscles, which cause its deeper colour. It is along this bark, that cylindrical vessels (Fig. *D. t, t, t, t, Fig. E. i, Fig. F. n,*) are observed to run lengthwise of the coral; which appear by the microscope to be parallel to each other, and out of which issue laterally other vessels infinitely small, (*E. t, t, t,*) which have a communication with the above-mention'd *membranulæ*. The use of these vessels is to give nutriment to the coral, by means of a milky juice, which they contain.

§ 23. The surface of this bark is slippery and uneven, when the coral has been just fished out of the sea; somewhat raised in some places, in others more depressed and flatted.

§ 24. Moreover, there are observed in several parts of the said bark little tubercles or prominences, (Fig. *A. s,*) which may be seen even without a microscope. These tubercles are pretty large at their bottom or basis, and round (Fig. *I, n, n,*) grow somewhat narrower towards their upper part (*o*), and terminate in a lip of some thickness, regularly divided into

into eight parts (*I. s, s,*) (*G. s, s,*) more or less even; which form the mouth (Fig. *I. t, G. t, H. a,*) of each tubercle, or, to speak more properly, of each cellule. The bark of the coral ends at the extremity of these parts: and thus it is, that all the inward part of each cellule of the white pellicle is formed.

§ 25. The white pellicle (*D. g, E. n, n,*) is doubled in some places, and forms a little bag (Fig. *F. s, c,*) which lines the inside of each cellule (*F. t,*) that is, to the beginning of the lip, or, we may say, to about the middle of the cellule.

§ 26. The substance of the coral (*F. o,*) gives way to the cellule by small cavities: yet these are not very visible in the old thick branches, but they are pretty easily seen in the young and slender (*B. a, c,*). Thus the cellule does not end at the coralline substance; since the white pellicle (*F. s,*) is between it and the said substance. The hollow of the cellule grows narrow into a sort of cone, (*F. t,*) with an obtuse *apex*; the belly of which is greater in diameter than the basis.

§ 27. The bottom of such a cellule faces the foot of the coral, and its mouth the branchy or most distant part from the foot. In this cellule is lodged the *polypus*, which is visible to the naked eye, (Fig. *A. s,*) but its exact shape is only to be seen by the microscope; and it was by this means, that I have been enabled to make a drawing of it.

§ 28. Wherefore it is from each cellule (*F. t, c,*) that a white, soft, and somewhat transparent *polypus* (Fig. *M.*) comes forth, or extends itself; which, in shape, resembles a star with eight equal rays, nearly conical, (Fig. *P.*) and furnished with other conical  
 appendices

appendices (*P. a, a, M. a, a,*) which issue out of it on both sides. The two rows of these have their direction nearly on the same plane. The rays are somewhat flattened, (*M. c, c,*) and a trough (*N. c, M. n, o,*) rises out of their center, somewhat widened at its beginning, with an opening or great mouth at top (*M. n,*). In its sides there are eight upright ridges, broad and elevated, and as many wrinkles or furrows; and each ray is inserted between every two wrinkles (*M. a, a,*).

§ 29. This trough is placed upon a smooth part, (Fig. *N. g,*) which we may call the belly of the animal; and this part, while the animal lives, and has not been hurt, is always erect in the cellule; tho' it be intirely disengaged, and separated all sides, from the said cellule; as may be plainly seen in some positions of the *polypus*.

§ 30. All these particularities are to be seen only when the coral is just drawn out of the sea, and suffer'd to stand in some of the sea-water: for, if you take the coral out of this water; or even if you do but touch it in the water, the *polypus* immediately retires into his cellule. In retiring, it contracts itself, the trough is closed up (Fig. *M. n, o,*) and each ray, (*Q. c,*) as also each appendix (*Q. a, a, a, a,*) shrinks, and enters into itself, just as snails pull in their horns: each ray pulls in about half its length, and with their ends they adapt themselves to the edges of the trough (Fig. *T. R,*).

§ 31. It is in this posture, that the *polypus* is seen the moment the coral is drawn out of the sea. The *polypus*, in this contracted state, seen without a microscope, resembles a drop of milk; and this is what

all the good coral-fishers take for the real milk of the coral; the rather, because, by pressing the bark of the coral with the fingers, the *polypus* is forced out, and, in coming forth, it always retains the appearance of milk. And this makes me believe likewise, that the accurate Andreas Cæsalpinus, who was the first observer of milk in coral, in reality saw nothing but the *polypi* in the likeness of milk.

§ 32. Altho' the *polypi* have their belly (Fig. N. g.) quite disengaged from the cellule, as we have said above; yet they always keep it therein, shortening and widening it so, as to make it bigger than the mouth or opening of the cellule (S. g.): and this may be seen very distinctly, by separating the cellule and its *polypus* from the substance of the coral, and then observing it on the back part. In this attitude it is, that we see, not only the belly very much shortened (Fig. S. g.) but also the posture, in which the *polypus* keeps itself in its own habitation.

§ 33. At the bottom of the belly (N g.) of some *polypi*, I have observed some roundish *hydatides*, extremely small and soft, transparent, yellowish, or tending to pale. The situation and figure of these *hydatides* induce me to believe, that they are the true eggs of the *polypus*.

§ 34. Altho' the size of these eggs is not much above the fortieth part of a line, yet, by the assistance of a good microscope, I think I have discover'd therein some vestiges of little grains, like those, which are common on the bark and substance of coral. These eggs are detached from the *polypus*, and, being soft, they adapt themselves, and stick to the hard bodies, on which they fall. Afterwards they spread

at

at the foot, or bottom, and swell up a little (Fig. *K*,); and, in this case, we very well discern an inward cavity in them, the upper part of which becomes uneven by eight wrinkles (Fig. *L*,), but is not open as yet. Shut up within this cavity the *fœtus* of the *polypus* remains, contracted within itself, and, as it were, without form. In due time the *polypus* grows ripe, and, as it were, adult; and then, the upper part (Fig. *I. s, t, s*,) opening, it comes forth properly extended (*N. g*,), and thus furnishes the coral with nutriment.

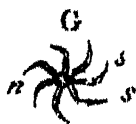
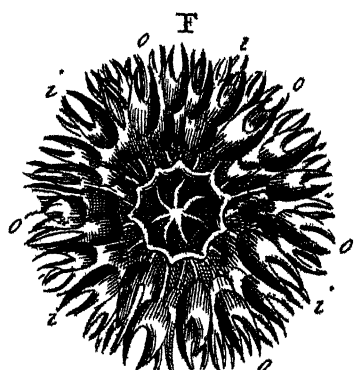
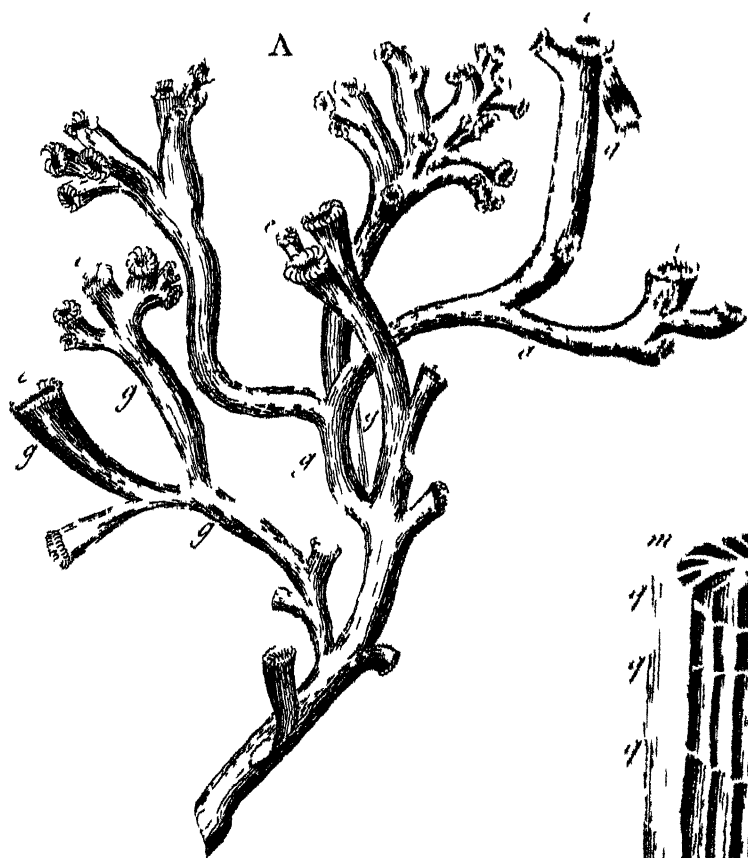
§ 35. While the first cellule is shut up, (*K*,) or the egg of the coral is in its substance, we do not find any one hard part in it like bone or marble; it is all soft: but afterwards, when the cellule opens, we begin to observe some hard *lamellæ*; and when it is grown bigger, and arrived at the height of about a line and half (Fig. *O*,), it widens at bottom, (Fig. *H. n*,) and at the top, (*H. a*,) and grows narrower in the middle, (*H. o*,) assuming the proper consistence and hardness of coral. And as this grows, (*II*,) the *polypi* are multiplied, and new branches of coral are formed.

§ 36. Here then we see the vegetation of a plant, and the propagation of an animal. It is submitted to the learned to decide at present, whether the coral belongs to one of these kingdoms rather than to the other; or whether, with greater justice, it deserves an intermediate place.











*Description of the Madrepora.*See Tab. III. Fig. *A*.

This is intirely like the coral, as to its hardness, which is equal to bone or marble. Its colour is white, when polished. Its surface is lightly wrinkled, and the wrinkles run lengthwise of the branches. Its inside is of a particular organization; having in the center a sort of cylinder, (Fig. *D. i*,) which is often pierced thro' its whole length by two or three holes.

From this cylinder are detached about 17 *laminae*, (Fig. *D. k, k*,) which run to the circumference in straight lines (Fig. *D. m, m, m, m*,).

These *laminae* are transversely intersected by other *laminae*, (Fig. *D. q, q*,) which form many irregular cavities throughout the whole plant. The branches (Fig. *A. g, g*,) are conical; and the basis of the cone is formed by the summit of the branch (Fig. *A. e, e*,). Every one of these summits has wrinkles on its outside, which run in the longitudinal direction of the branches (Fig. *B. c, c*,); and each wrinkle answers to a *lamina*, (Fig. *C. e, u, e, u*,) and each *lamina* is of the shape of a prism, (Fig. *E*,) the basis of which is warty, and faces the outside, (Fig. *C. e, u*,) and its point is cut into teeth, (Fig. *E. n, n, n*,) and belongs to the inside. The cellule, (Fig. *B. a, a, a, c, c*, Fig. *C. e, e, u, u*,) which is of the shape of a chalice, is composed of these *laminae* ranged into a circle.

In every one of these cellules is found a little *polypus*, represented in Fig. *F*. but considerably magnified; the mechanism of which is this :

O

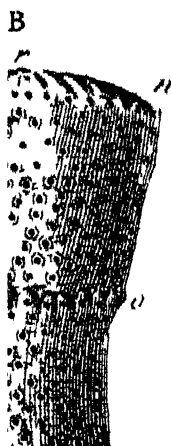
Three

Three different parts, unlike each other, compose this animal; *viz.* the feet, (Fig. *F. o, 1,*) a trough, (Fig. *F. 8, H. t,*) and an head, (Fig. *G. n.*) Each foot begins by two conical appendices (Fig. *H. o, o, o,* Fig. *I. o,*). By the union of these appendices a rounded part is formed, which, in some degree, resembles the belly of a muscle, (Fig. *H. i,* Fig. *I. x*) by means of which the foot is shortened and lengthened. To this part (Fig. *I. x,*) is annexed a little cylinder, (Fig. *I. n,* Fig. *H. c,*) the length of which is indeterminate.

These feet are ranged all around in great number, and annexed to the *laminæ*, (Fig. *B. a, a, c, c,*) and are all united to the trough, (Fig. *H. c,*) on the outside of which are seen ten cavities, with an equal number of prominences (Fig. *H. t, t, t, s, c, c,*) and in these is lodged the animal's head (Fig. *G.*) which has prickly rays, the precise number of which I could not determine, on account of the extreme velocity of the continual oscillatory motion of the head from right to left, and from left to right: yet I thought I could perceive the number of these rays to be eight: and the use of them may be for the animal to catch and hold its food. This part is not always to be observed, because it sometimes hides itself, by closing up the trough (Fig. *H. s, s, t, c,*) about it; and, by thus covering itself, it is safe in its habitation.

As the figure of this animal bears no resemblance to the *urtica marina*, I cannot see, how one could class the *polypus* of the *madrepora* with the *urtica*.

This animal is extremely tender, and generally transparent, and very beautiful for its variety of colours. I have observed it in spring and autumn in the





the neighbourhood of Novigno and Orsera, where it is often fished up.

*A Description of the Miriozoon, or Pseudoforalium album fungosum of Aldrovandus.*

As the size and shape of this polypary is sufficiently seen in Fig. *A*. I shall describe only what the microscope has enabled me to observ'd therein; and what Count Marfigli, tho' peculiarly diligent, has either overlook'd, or examin'd with too little attention. And this I do the more willingly, as the mechanism of this body to me appears very wonderful.

Its substance is rather like that of bone than of marble, but brittle withal: and its brittleness proceeds from the great number of cellules, with which it is hollowed.

These cellules are ranged all around in the branches, (Fig. *C. n, m,*) and disposed in the manner of a quincunx; (Fig. *B. n, o,*) and I don't know to what better to compare the form of each cellule, than to one of those cinerary urns, which are frequently found in Italy (Fig. *E. i*).

In each of these cellules lodges an oblong *polypus*, (Fig. *G.*) slender at the tail, (Fig. *G. t,*) thick at the belly, (Fig. *G. e,*) and again slender at the neck, (Fig. *G. c,*) to which is attached a little cover, (Fig. *G. o,* and *F. o,*) round, concavo-convex, and of a bony substance. This cover is attached by its lower part (Fig. *F. n,* and *H. e,*) to the entry of the cellule (Fig. *F. x*).



When the *polypus* chuses to spread itself out, it opens the cover, and out of its neck (Fig. *H. s, s,*) thrusts an ample proboscis, (Fig. *H. g,*) which is in the shape of a cup; and with this it probably takes its food. There are two little muscles (Fig. *H. a, a,*) at the lower part of this proboscis, which are attached to the cover.

When the animal returns into its nich, the proboscis sinks into itself; and the animal, by contracting itself, draws back the cover; and thus the cellule is perfectly closed, and the creature secure in its retreat.

However, all the *polypi* of this plant do not enjoy this conveniency and security, but only the adults; that is, those, which dwell about the branches. As for the others, that are not as yet adult, and live and lodge on the tops of the branches, (Fig. *B. r, n, n,* and *D. n, x,*) they have no covers; and a considerable number of them dwell in imperfect cells, or in such as are finished but in part, (Fig. *D. t, t,*) and made of a sort of cartilaginous and membranaceous materials. The imperfection of these cellules, and the weak consistence of the paste, which forms them, afforded me a plain proof, that the cellules are the work of the *polypi*, as the niches, wherein they lodge, are made by some shell-fish.

XV. *A Dissertation upon the Class of the*  
*Phocæ marinæ, by James Parlons M. D.*  
*F. R. S.*

Read Feb. 14. 1750. **I**N February 1742-3, I had the honour to give this learned Society some account of the sea-calf, which was shew'd at Charing-cross at that time, which I often saw whilst alive, and afterwards opened it. It is printed in the 496th number of the Transactions, p. 383.

There is also now in town a seal (another species of *phoca*) alive; which gives occasion to my taking farther notice of this class of animals, that the Society may have a clear idea of their differences, and great variety; and also be undeceiv'd with respect to the sex of this, which is now in town.

All the species of *phocæ*, this being the generical name, have among them a very great likeness to each other, in the shape, not only of their heads, but also of their bodies and extremities. They are webbed nearly alike, are alike reptile, viviparous, bringing forth, suckling, and supporting their young alike; and, in fine, all have the same title to these appellations, *phocæ*, *vitulus marinus*, *sea-cow*, *sea-lion*, &c. and these names are vulgarly given to them, as their size happens to be greater or smaller; and the first of these names from *φῶκη*, or, according to Dr. Charleton, from *βῶκη*, signifying a noise, or kind of grunting, which they all at some times exhibit.

As to the sex of this present creature, the owner reports it to be female. Now that, which I have  
 already

already described being a female, I was the better able to see how far the present *phoca* differs from that. And indeed, altho' I will not absolutely pronounce it a male at present, yet, I confess, I had much rather incline to think it so, than otherwise, for the following reasons: 1. Tho' I several times went to view it with the greatest care, held it up by the tail, and turn'd it as I thought proper, I could discern no *pudenda* like that of the former; there being no aperture under the tail, but the *anus*. 2. I could not discern any mamillary vestiges in the least upon any part of the belly; and, 3. at some distance behind the umbilical regions, about the place where one would most naturally expect to find a *penis* in the males of viviparous animals, there is a roundish vent or hole, not at all projecting from the body. Now the want of the marks above-mention'd seems to bespeak the following query; Whether this vent may not probably be the place, from whence a *penis* is protruded upon proper occasions? because, if this animal had a *penis*, which, like that of other animals, was prominent from the surface of the body, it would be torn to pieces; as he is a mere reptile, dragging his hinder parts along shores, rocks, stony places, sands, and such-like, when he is out of the water, being in no-wise able to raise his body from the ground by the posterior webs; and therefore it is more reasonable to suppose the *penis* concealed, and occasionally capable of extrusion; and no author, that I know of, makes any mention of this matter to satisfy the curious about it. And as to what the servant, attending the *phoca*, reports, concerning a menstrual discharge from it, I have examined him about it,

### [ III ]

it, and he knows no more, than that he finds, sometimes, swimming upon the surface of the water, wherein the *phoca* lives, a white froth; and when he takes it up, it dwindles away. This he supposes to be the menses; which I suppose to be a froth, brought on by the action of the animal upon the salt and water, which are very susceptible of such an effect. But, if a shew-man gives out, that his animal is a sea-lioness, he will easily report any other absurdity.

The different species of this class, or rather *genus*, of animals, are distinguishable, by their proportion, their size, as to their full growth, their teeth, webbed feet, and whatever other parts in some may not be proper to others.

As to the first, this species before us is shorter and thicker in proportion than that I described before; which appears by the figure in the before-mention'd transaction; and it may be depended on, that I was as exact as possible in taking its true proportion. Dr. Grew, in his excellent book of the *Rarities*, &c. mentions a difference in the proportions of two which he describes, in their thickness; that presented to the museum by Mr. Haughton being thicker than the other. He also gives an account of another species, which he calls the long-necked seal, in these words; "He is much slenderer than either of the former; "but that, wherein he principally differs, is the length "of his neck; for, from his nose-end to his fore- "feet, and from thence to his tail, are the same "measure; as also in that, instead of his fore-feet, "he hath rather fins; not having any claws thereon,

“ as have the other kinds.” The head and neck of this species are exactly like those of an otter. One of those, which is also now in our museum, taken notice of by the same author, has an head shaped like that of a tortoise ; less in proportion than that of every other species, with a narrowness or stricture round the neck : the fore-feet of these are five-finger’d, with nails, like the common seal.

Their size, as to the utmost growth of an adult, is also very different. That before described, was 7 feet and an half in length ; and, being very young, had scarce any teeth at all. This in town is but about 3 feet long, is very thick in proportion, and has a well-grown set of teeth ; which, in a great measure, shews this to be about its full growth. The *manati* is also a *phoca*, and is one of those species, which grows to a prodigious size. The great skin, in the museum, is that of a *manati* ; which seems to me to agree with the other species of this family, in every essential part, except broad bifid webs, instead of webbed feet : and Peter Martyr gives an account of one of these, which was thirty-five feet long, and twelve thick.

The docility of this seal in town is, with reason, much admired, as a thing unusual and strange to us ; but it appears, from Dr. Charleton, that, in his time, it was not uncommon for the seamen and fishers to catch some of these creatures sleeping, on the coasts of Cornwall and the Isle of Wight, and bring them to be so tame, as to get money by shewing them, and their performances : and he adds, that the people of the former place call the larger kinds about that coast *soils*, and the smaller *seals*.

But

But the story told us, by the above author Martyr, of that great *manati* shews how capable these creatures are of being render'd very familiar; and how susceptible of impressions, tho' they really seem as unfit for any kind of education as any other whatsoever.

This author describes the *manati* very fully; and then tells this remarkable story:

“ A governor, in the province of Nicaragua, had  
 “ a young *manati*, which was brought to him, to  
 “ be put into the lake Guanaibo, which was near his  
 “ house; wherein he was kept for the space of  
 “ twenty-six years; and was usually fed with bread,  
 “ and such like fragments of victuals, as people often  
 “ feed fish with in a fish-pond. He became so familiar,  
 “ by being daily visited and fed by the family,  
 “ that he was said to excel even the dolphins, so  
 “ much celebrated by the antients for their docility  
 “ and tameness. The domestics of this governor  
 “ named him Matto; and at whatsoever time of  
 “ the day they call'd him by that name, he came  
 “ out of the lake, took victuals out of their hands,  
 “ crawl'd up to the house to feed, and play'd with  
 “ the servants and children; and sometimes ten persons  
 “ together would mount upon his back, whom  
 “ he carried with great ease and safety cross the  
 “ lake.”

All that is here mention'd of the docility of this *manati*, does not much surpass that of this seal in town. He answers to the call of his keeper, and is observant of his commands; takes meat from his hand, crawls out of the water, and stretches at full length, when he is bid; and, when order'd, returns

into the water; and, in short, stretches out his neck to kiss his keeper, as often, and as long, as required. These are marks of a tractableness, which one could hardly expect from animals, whose mein and aspect promise little, and indeed whose places of abode, being for the most part inaccessible, prevent their being familiarized to any commerce with men, except by mere chance.

The teeth are very well preserved in the skin of the *manati* in the museum: they are 16 in the upper, and 14 in the under jaw; and of these, 4 are between the canine teeth of the upper, and 2 between those of the under jaw. They are all conical from the gums; the canine teeth are two in each jaw; being an inch and half long each, and of the same form with the rest; and they all bend a little backwards by a small curve in themselves. Nor have the very back teeth of all the least resemblance to the *molars* of other animals.

The *walrus*, or *mors*, is another species of *phoca*, and differs very little in shape and parts from the other species of this *genus*; except that the two canine teeth of the upper jaw are of a prodigious size, like the great teeth of an elephant.

There are some species of this *genus* of the *phoca*, which never grow to above a foot long; and there are of all sizes at full growth from these to the *manati* and *walrus*. The skins of every species have short hair, and their colours are variegated from the straw-colour and yellow to the deepest brown and black. They are sometimes regularly brindled, sometimes curiously spotted; sometimes in brown clouds upon a yellow ground, like that of a pied horse; and sometimes the brown or black occupies the

the greater part of the skin, having less of the yellow; and, in short, even those of the same species are as variously spotted or clouded as the hounds in the same pack; and it is probable, that, in unfrequented islands and countries, other species of this tribe are yet undiscovered. But it must be observed, that, where no other difference, but the variation of the colour, appears among them, that is, in their size, proportion, teeth, or extremities, they are no more to be accounted different species, than cows having various changes in the distribution of the clouds or spots on their skins.

The reverend Mr. Walter, in the first chapter of the second book of *Lord Anson's Voyage*, describes an animal under the name of the sea-lion, an abstract of which you have as follows. He says, That seals haunt the Island of Juan Fernandes in great numbers; but that there is another amphibious creature here, call'd a sea-lion, that bears some resemblance to a seal, tho' much larger: they are in size, when at full growth, from 12 to 20 feet in length, and from 8 to 15 in circumference. Their skin is an inch thick; and they have at least a foot in thickness of fat upon the flesh; so that a large one affords a butt of oil: and he adds, that one being first shot, they measured the quantity of blood, having cut his throat for that purpose, and it amounted to two hogheads, besides what remain'd in the blood-vessels. Their skins were cover'd with short hair, of a light dun colour; but the tails, and their fins, which serve them for feet on shore, are almost black. Their feet are divided at the ends like fingers; the web, which joins them, not reaching to the extremities; and each of



these extremities is furnished with a nail. They have a distant resemblance to an over-grown seal; but in some particulars there is a difference; especially in the males, who have a large snout or trunk, hanging five or six inches below the end of the upper jaw. The females have not this; and the males are much larger than the females. The larger males engross to themselves a great many females; intimidating the smaller, and driving them away. This author says, they live at sea all summer, and on shore in winter. They may indeed meet more frequently in herds at one time of the year than another; but such is their nature, that they cannot totally inhabit the waters, having great need of respiration in the open air; and therefore they must frequently come out of the water to rest, as well as to feed on such herbage as is proper for them. He adds, that they ingender and bring forth their young, generally two at a birth, while they live on shore; which, says he, being about the size of full-grown seals, they suckle. On shore they feed on the grass and verdure on the banks of fresh-water rivers, and sleep in herds in miry places. They are sound sleepers; and therefore some of the males place themselves at certain distances to watch the sleeping herd; and are capable of alarming, by grunting or snorting like horses. The males fight one another furiously about their females, wounding one another desperately with their teeth. They kill'd several of these for food, especially for the hearts and tongues, and esteemed them preferable to those of bullocks\*. He represents them as very  
full

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\* That these are wholesome food, and have frequently been used as such in former times, in England, appears from

full of revenge and fury in defending their young ; for that one day a sailor being carelessly employ'd in skinning a young sea-lion, the female, who was its parent, stole upon him, and laid hold on his head, wounding him with her teeth in so desperate a manner, that he died in a few days.

This history may be applicable to other species of *phocæ*; and, by this description, as well as the figures exhibited

from the following note, sent me by the reverend Dr. Jeremiah Milles.

S I R,

THE dean of Exeter desired me to transcribe the two following notes from the 6th volume of *Leland's Collectanea*, and to send you them.

Page 1. in the account of the quantity of provisions purchased for the enthronization-feast of archbishop Neville, there is this article,

*Porpoises and Seals XII.*

and yet, in the bills of fare, which were added afterwards, I find not the least mention either of porpoise or seal, served up or dressed in any shape.

So likewise, in page 31 of the same volume, where is an account of the provisions, and their prices, which were bought for archbishop Warham's enthronization-feast, there is an article,

*De Seales & Porpoiss. prec. in gross 26 s. 8 d.*

and yet the preceding bills of fare do not mention either of these fishes, as served up at any of the tables of the feast. Were they not bought for the dinner of the servants, and other inferior officers ; whose bills of fare are not particularly mention'd in this account ?

If these notices are of any use to you, I shall be very glad to have communicated them ; and am, Sir,

Grosvenor-street,  
Feb. 15. 1750.

Your very humble servant,

Jer. Milles.

exhibited in the book, what are counted sea-lions, are *manati's*.

It will be well to observe, that, in the figure of the male, which, Mr. Walter says, was taken from the life, there is no appearance of a *penis*: which would, in some measure, support our opinion of the sex of the seal in town. For I should imagine, that, if a *penis* was to be seen, the draught's-man would scarce have omitted so remarkable a part.

The learned Linnæus ranks this *genus* of animals with those of his second order of quadrupeds; and indeed with great propriety, however injudicious it may lately have been thought: for, altho' none of this tribe can use the posterior extremities to raise themselves up, or stand upon them, as upon legs and feet; yet they swim and guide themselves in the water with them; for which they claim the title of *palmipedes*, or webbed feet; for they have no similarity with fins.

If it be objected, that these animals would come more naturally under his class of *amphibia*; we may assert, that he had two very good motives for ranking them with quadrupeds. First, he had our great Ray for his director, who has himself done the same thing: and, secondly, he found, that, altho' these creatures are really amphibious, yet, the commanding characters, by which he has, with great sagacity, distinguished his classes, prevail here to give them a place rather among the quadrupeds than the *amphibia*.

This great naturalist divides the animal kingdom into six classes, and each class into six orders. Each order is again divided into different *genera*, and each  
*genus*

*genus* again has its different species. The *phoca* then is the sixth *genus* under the second order of the *quadrupedia*; which order is that he calls *feræ*. It is very necessary to give this detail, in this place, of his glorious method, that we may do justice to so great a man, and also introduce the commanding characteristics by which animals fall naturally in the places which he allots them, as far only as it relates to this tribe under consideration.

To the class of *quadrupedia* he gives these general characters :

*Quadrupedia* { *Corpus pilosum* — *pedes quatuor*.  
                   { *Femina viviparæ, lactiferæ*.

To the second order of this class, the *feræ*.

*Feræ* { *Dentes primores utrinque sex*.  
           { ——— *canini longiores*.

To the sixth *genus* of the *feræ*, the *phoca*.

*Phoca* { *Dentes primores superiores sex, inferiores 4.*  
           { *Pedes 5. 5. palmati natatorii*.  
           { *Auriculæ nullæ*.

Under these characters he ranks but two general species :

1. *Phoca dentibus caninis tectis* — *Seebund*.
2. *Phoca dentibus caninis exsertis*. — *Walrus*.

The different species of the former, whose canine teeth are hid by the lips, are more numerous than those, whose superior canine teeth, as those of the *walrus*, project from the lips, and are, from their  
variations

variations in other parts, to be described by their particular marks, besides the canine teeth. We shall therefore add to these two species of Linnaeus some particular ones, which he had not seen (specimens of which our museum affords); and shall make a table of them; that whenever any others shall at any time occur to the learned, they may be added to these.

*Phoca minor, dentibus caninis testis, palmis anterioribus digitatis, ungulatis, posterioribus latis ordinariis, ungulis teretibus donatis.*

The common seal, <i>Vitulus marinus</i> , Sea-calf,	}	Charleton, and many other authors.
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On the shores of the Isle of Wight, Cornwall, and almost every country.

*Phoca minor, dentibus caninis testis, cervice longiore, capite lutræ caput referente, palmis anterioribus latis non digitatis, posterioribus latis ordinariis.*

The long-neck'd seal. — Grew.

On the shores of divers countries.

*Phoca minor, dentibus caninis testis, palmis anterioribus digitatis, ungulatis, posterioribus latis ordinariis, collo constricto, capite testudini-forme.*

The tortoise-headed seal.

On the shores of many parts of Europe.

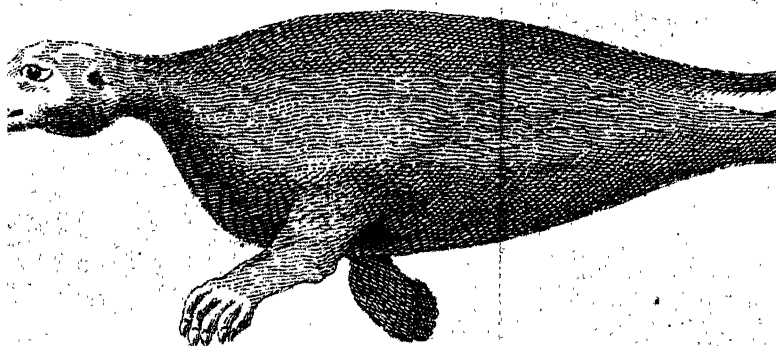
*Phoca*



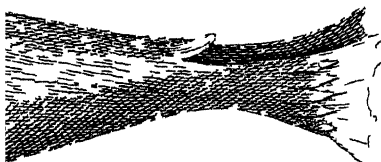


*The*

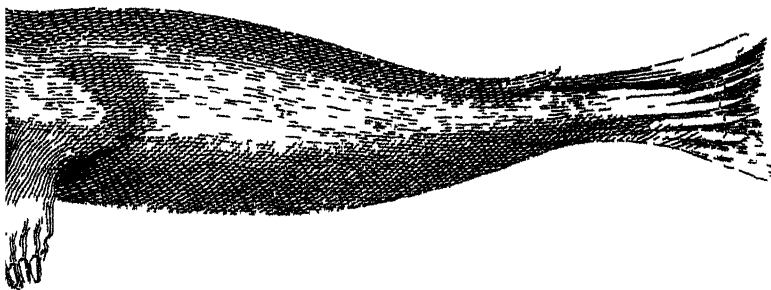
*The Tortoise-headed Seal. p. 120*



*p. 120.*



*The common Seal. p. 120.*







*Phoca major dentibus caninis testis, palmis anterioribus digitatis, ungulatis, posterioribus latis ordinariis, corpore longiore.*

The long-bodied seal.

On the coasts of Cornwall, and the Isle of White. Described and figured in the Transactions, No 469, p. 383.

*Phoca major, dentibus caninis testis, palmis anterioribus digitatis, ungulatis, posterioribus latis bifidis.*

Manati. — De Laet. Pet. Martyr.

Sea-lion. — Lord Anson.

Nicaragua, and other American coasts, and the Island of Juan Fernandes.

P. S. Monsieur de la Condamine, in the account of his voyage down the River of the Amazons, describes an animal, which, without doubt, is a species of the *phoca*: his account is as follows:

“ I drew from the life the largest fresh-water  
 “ fish, which the Spaniards and Portuguese have  
 “ call’d the sea-cow, or ox-fish; which must not be  
 “ confounded with the *phoca*, or sea-calf. This  
 “ fish feeds on herbage on the brinks of rivers: the  
 “ flesh and fat are very like that of veal: the fe-  
 “ males have duggs to suckle their young withal:  
 “ some have made this fish resemble too nearly  
 “ the ox, by attributing horns to it, which na-  
 “ ture never designed it. It is not amphibious,  
 “ properly speaking, for it never goes intirely out  
 “ of the water; being incapable of doing so;  
 Q “ having

“ having only two fins near the head, like wings,  
 “ sixteen inches long, supplying the places of arms  
 “ or legs. He only stretches his head out of the  
 “ water to reach the grafs. This, which I drew,  
 “ was female, and was about seven feet and half  
 “ long; its greateft thicknefs being but two feet.  
 “ I have fince feen of thefe much larger. The  
 “ eyes of this animal bear no proportion with the  
 “ fize of the body; they are round, and are but  
 “ three lines in diameter. The openings of the  
 “ ears are yet lefs, appearing like pin-holes. Some  
 “ have thought this fifh was peculiar to the River  
 “ of the Amazons; but it is as common in the  
 “ Orinoque; and is found alfo in the Oyapoc,  
 “ and many other rivers about Cayenne, and the  
 “ coasts of Guyane, and elfewhere. This is what  
 “ is call’d Lamentin, at Cayenne, and in the French  
 “ iflands of America; but I believe it a fpecies a  
 “ little different. It never is feen out at fea; and  
 “ it very rarely is at the mouths of rivers; but we  
 “ find it above a thoufand leagues from the fea in  
 “ moft of the great rivers, that fall into that of the  
 “ Amazons, as in the Guallaga, the Pastaga, &c.”

Whoever confiders this ingenious author’s accurate account of this animal will eafily, and with great propriety, be able to range it with one of the fpecies in our account above.

**XVI.** *An Account of an iliac Passion, from a Palsy of the large Intestines; communicated to Dr. De Castro, F. R. S. Translated from the Latin, by Tho. Stack M.D. F. R. S.*

Read Feb. 21.  
1750.

**A** MERCHANT, aged seventy, who had been accustomed to hardships from his infancy, was, for the last six years, very subject to rheumatic pains; but, looking on his disorder as the effect of old age, he rejected all medical advice. In these circumstances it happened, that he was suddenly set upon by a party of soldiers, who, with severe threatnings, turned him out of his house, and took possession of it: which so terrified him, that he was seized with a violent belly-ach; and his agony so overpower'd him, that he fell on the ground half-dead; and at the same time he voided blood by the *anus*.

When his fright and grief for the loss of his substance were over, he return'd to his usual way of life, and was much subject to the gripes all the ensuing winter, which he took no care of. During this time, he suffer'd much from costiveness, till March 1747, when he was seized with severe pains about the navel; and tho' he had clysters of several sorts given him, not one of them could be made to pass. He was feverish and thirsty, with a white moist tongue, and could not sleep. He was blooded as much as he could well bear; and the blood did not appear inflammatory. He was treated with laxative medicines, antiphlogistic fomentations, and every

thing, that could be thought proper, to ease the gripings, and give a free passage: but nothing took effect for seven days together.

On the eighth he began to break wind, retain the clysters, discharge some little *stercus*, and to sleep, tho' not quietly; and, on the ninth, to make turbid urine. But these promising appearances were but of short duration; for, on the eleventh, his belly was so bloated, that he seem'd tympanitic; and an acute pain, which he had in the hypogastric region, darted up towards the midriff on the right side: and now the *mucus* of the intestines came away with the clysters. He had bad sweats, and made foul urine, without sediment.

On the 15th a consultation was held; and, as his thirst and fever were abated, and the medicines hitherto prescribed for opening a passage, and taking down the swelling of the belly, which seemed ready to burst, had proved ineffectual, it was agreed to make him swallow six ounces of crude quicksilver, with oil of sweet almonds, and syrup of violets; and, soon after, to throw in several purging clysters.

In nine hours a passage was opened, and he voided much black liquid excrement, without the least grain of quicksilver, tho' very carefully sought for. A little after that, he vomited much; and, in what he threw up, there plainly appear'd excrements, and globules of mercury. This was soon follow'd by thirst, a little slow fever, very troublesome gripings, no sleep, red high-colour'd thick urine, in very small quantities, breaking of wind without any ease, vomiting of every thing he took, great weakness, and partial

tial sweats in the forehead and breast. Under these symptoms he languish'd to the twentieth day, and then died.

The appearances, upon dissection, were these : The *omentum* was consumed ; but the *colon* was inflamed in several places, and so distended with wind, that it nearly filled the whole abdominal cavity. Its ligaments or bands were so thoroughly effaced, that there was not the least sign of them remaining. In like manner, the *cæcum* was so vastly stretched, as to take up the whole capacity of the *pelvis* ; and that part of it, which is touched by the thick gut, was gangrened, and perforated with a small opening. Having clear'd it of the excrements, there were no internal *rugæ* at the insertion of the *ileum*, nor any traces of the valve of the *colon*, or of its braces, to be observed. For it was quite smooth on the inside, as well as the *colon*, by the destruction of the cellules, which it has in a natural state. The quicksilver was dispersed all over the cavity of the *abdomen*, in such quantities, that it was easy to perceive, that none had been discharged by stool. Every thing else, contained within both the cavities, was in its natural condition.

XVII. *A Letter from the Secretary of the Royal Academy of Sciences in Sweden, to Cromwell Mortimer, M. D. et R. S. Sec. concerning the variation of the magnetic needle.*

Celeberrimo Domino Doctori, et Societatis Regiæ Londinensis Secretario, Cromwello Mortimer, S. P. D. Petrus Wargentini, Acad. Reg. Scient. Suevicæ Secretarius.

Read Feb. 21. 1750. **O**BIIT ante paucos menses secretarius Academiæ Regiæ Scientiarum Suevicæ, vir in mathematicis scientiis versatissimus, D. Petrus Elvius : cui, ex decreto academiæ, ego mox suffectus secretarius, muneris mei esse judicavi, commercium literarium cum exteris societatibus, academiis, et viris eruditis, instituere, cum persuasissimus sim ejusmodi literatorum commercia plurimum ad scientiarum incrementum facere.

\*\* Ut aliquid ad scientias pertinens tibi imperitiam, paucis narrabo de observatis a me nuper variantibus quotidie paullulum, sed sæpe admodum turbatis, declinationibus acus magneticæ.

Halleius vestras dudum suspicatus est, esse quoddam inter lumen boreale et acum magneticam commercium. Id certissimis experimentis et observationibus evicerunt jam ante aliquot annos Celsius atque Hiorterus, astronomi apud nos, dum viverent, celebres, qui sæpissime animadverterunt, acum magnopere turbatam atque inquietam esse, quoties lumen  
boreale

boreale ad zenit, vel ad plagam cœli meridionalem ascendit, ita quidem, ut declinatio videretur sequi motum luminis, et intra pauca temporis minuta totos tres et quatuor gradus aliquando variare. Res fide major mihi initio visa est. Meis oculis tam mirum phænomenon notare cupiebam. Cum itaque mihi traderetur acus, pedem Suecanum longitudine æquans, ab opifice nostro ingeniosissimo D. Ekstrom confecta, agilissima; mox, ineunte Februario hujus anni, cœpi annotare illius declinationes; quas statim quotidie variantes deprehendi, prout Grahamus, Celsius, etc. antea observaverant, ea videlicet lege, ut acus ab hora septima matutina ad secundam post meridiem, ab oriente ad occidentem magis magisque discedat, interdum tertiam vel quartam partem unius gradus. Post horam secundam iterum revertitur ad octavam vespertinam, usquedum eundem fere situm attigerit, quem hora octava matutina. Per totam noctem fere quæta esse solet, saltim non nisi parum circa mediam noctem abit ad occidentem, mox ineunte mane reditura. Hæc diurna variatio nunquam fallit, sed constans et fere regularis est, nisi lumen boreale impediat.

Cum acus hoc modo, a die 6 Februarii ad 15<sup>m</sup> circa septimum gradum declinationis \* occidentalis vagata esset quotidie, eluxit, die 15<sup>o</sup>, lumen boreale, non tamen admodum vividum. Magna cum voluptate percepi, acum mox affici, ut intra 10 temporis minuta,

\* Hæc declinatio non est vera et media hoc tempore Holmiæ, sed aliquanto minor vera. At hac occasione non quæsi veram declinationem, sed ejus tantum variationem.



minuta, circa horam decimam vespertinam, abiret 20' ad occasum et intra alia decem minuta rediret et discederet 37' ad ortum. Cessante lumine acquievit acus. Postero die insignis contigit turbatio, ideoque ipsas observationes citare non ingratum tibi esse judico, pro tota ista die.

Tempus h / m	Declinat. ac. g /	Tempus h / m	Declin. acus. g /
8 0 <i>a. m.</i>	7 0	10 56 <i>p. m.</i>	7 1
10 0	7 4	11 6	6 25
12 0	7 10	11 10	5 51
2 0 <i>p. m.</i>	7 15	11 19	6 43
4 0	7 11	11 22	6 26
8 0	7 2	11 26	6 42
9 0	6 50	11 37	5 23
10 0	6 8	11 45	5 0
10 5	5 31	11 58	4 35
10 8	5 47	12 0	5 0
10 15	5 29	12 15	6 30
10 30	6 0	12 27	6 22
10 46	7 26	12 35	6 55

Per totam hanc noctem vix aliquo momento quievit acus; sed, omnibus aliis rebus quietis, me solo tacitis passibus acum invasente, nullo ferro admoto, vagabatur hinc inde quasi vertigine correpta. Lumen boreale hac nocte fuit in plaga meridionali splendidum et vivacissimum, interdum per totum cœlum se rapidissimo motu diffundens: sed ego intentus acui, non fatis luminis apparentias observare potui. Sequentibus diebus admodum quietam mansit acus, ut et variationes diurnæ solito minores fuerint. At die 28 Februarii,

Februarii, novo erumpente lumine boreali insigniore. Sentiit id acus, quæ cœpit vacillare hora post meridiem quarta, sole adhuc splendente: unde intellexi nos proxima nocte visuros lumen boreale. Nec scellit eventus. At locus non permittit ipsas huc transcribere observationes: sufficit dixisse, quod vacillaverit acus inter  $6^{\circ} 50'$  and  $9^{\circ} 1'$ . Per totum mensem Martium nihil præter consuetas diurnas digressiones unquam animadverti, ne  $6^{\circ}$  quidem, licet lumen boreale tum conspiceretur, sed debile et quietum prope horizontem borealem.

At die secunda Aprilis, ruptis induciis, rursus exarsit lumen, acui infestans, idque per duos integros dies, die noctuque pariter, quantum ex acu cognovi; nam illa continuis agitata motibus fuit, licet lumen non nisi noctu observari posset. En præcipuas observationes.

	Tempus				decl.	ac.
	<sup>h</sup>	<sup>m</sup>			<sup>o</sup>	<sup>'</sup>
April 2,	2	40	<i>p. m.</i>	— — — —	7	7
	4	20		— — — —	7	10
	5	22		— — — —	7	21
	10	31		— — — —	5	35
	10	55		— — — —	5	57
	11	34		— — — —	6	27
	11	52		— — — —	6	0
	12	3		— — — —	4	56
	12	8		— — — —	5	27
	12	18		— — — —	6	34
	12	21		— — — —	6	18
	12	28		— — — —	6	37
	12	45		— — — —	6	22
April 3,	7.	0	<i>a. m.</i>	— — — —	7	5
				R		April

	Tempus				decl.	ac
	h	'			o	'
April 3,	10	15	—	—	6	48
	10	49	—	—	7	15
	3	30	<i>p. m.</i>	—	7	25
	4	43	<i>p. m.</i>	—	8	55
	4	49	—	—	9	55
	5	4	—	—	8	7
	5	11	—	—	8	38
	5	27	—	—	8	10
	5	37	—	—	8	37
	6	9	—	—	7	55
	7	8	—	—	7	22
	10	25	—	—	7	10
	10	43	—	—	8	29
	10	54	—	—	7	1
April 4,	7	14	<i>a. m.</i>	—	6	29
	8	5	—	—	5	54
	9	40	—	—	6	53
	9	50	—	—	7	22
	10	17	—	—	7	0
	10	53	—	—	7	54
	1	29	<i>p. m.</i>	—	7	11
	2	19	—	—	7	19
	2	46	—	—	6	29
	4	50	—	—	7	16
	6	52	—	—	7	2
	8	0	—	—	6	58
	10	15	—	—	6	55
	11	3	—	—	6	50

Variavit itaque acus intra diem unum plus quinque integris gradibus.

Die 20 Aprilis, cum toto die vehementer plueret, acus tamen turbata fuit continue, maximæ variationes

nes erant duorum graduum. Non conquievit acus ante meridiem dici sequentis.

Sed te jam nimis diu detinui, vir æstumatissime ; ideoque heic subsistens me ture amicitie tuoque favori etiam atque etiam commendo. Vale.

Stockholmizæ, calendis Maii,

1750.

XVIII. *An Extract of a Letter, dated May 2, 1750, from Mr. Freeman at Naples, to the right honourable the Lady Mary Capel, relating to the Ruins of Herculaneum.*

Read Feb. 28. 1750. **Y**OU remember, that, about 7 or 8 years ago, the discovery of Herculaneum was greatly talk'd of, and reported to have been swallow'd up by a violent eruption of Mount Vesuvius ; which, by the most accurate accounts, was in the first year of the reign of the emperor Titus, and 79 years after Christ.

The situation of this antient city is, as it were, at the foot of Vesuvius near the sea, and just at one end of the village of Portici, the palace of the king of Naples's summer residence ; and, I dare say, a great part of the city is under the said village.

I was first conducted down a narrow passage, which they have dug wide enough barely for two persons to pass by each other ; and descended, by a gradual slope, to the depth of about 65 feet perpendicular. Here I saw a great part of the ancient theatre, being a building in the form of an horseshoe. That part of it, where is supposed to be the orchestra

and stage, was not so cleared out, as to be distinctly seen: the other, where the spectators sat, is very visible, and consists of 18 rows of broad stone seats, one above another, in a semicircular form, and are sufficiently wide to place the feet of those, who sit behind each other; so that they may be said to be both seats and footstools. Altho' this theatre is not emptied of the matter or earth, that filled it, yet they have dug quite round the exterior part, by which one may judge of its spaciousness. At certain proper distances, within the circuit of the seats, thro' the whole range, from bottom to top, are little narrow flights of steps, by which the spectators might come to, or go from, their seats commodiously, without crouding. These steps or stairs also lead up, in a strait line, to a sort of gallery, several feet wide, which ranges all round the outside of the theatre, and which is called the precinct; above which there are other stairs, which lead to a second. By this precinct it is judged, that the theatre, with the orchestra, must be about 52 or 53 feet diameter.

I observed, going round the theatre, several large square pilasters, equally distant from each other; and which, doubtless, supported the whole edifice. These pilasters are of a thin compact red brick, adorned with marble cornices. The pavement of this theatre must have been very beautiful, by the different colour'd marble, that has been taken out of it, and some that remains. In short, by the broken pieces of cornices, mouldings, and carved work, and the many fragments of pillars, &c. which have been found within and without the theatre, it appears to have been a most magnificent edifice.

There

There are two principal gates to the theatre, with inscriptions on the architraves, which are taken out, and placed in the king's palace, among the other curiosities. Those, who have the care of all, will not suffer one to copy any thing: however I lagged a little behind the rest of the company, and copied, from the perfectest of the two architraves, the following inscription ;

LANNIVS.MAMMIANVS.  
RVFVS.II.VIR.QVINQ.  
THEATR.OP.NVMISIVS.  
P.F.ARCH.EC....

The antiquarians will have it, that Mammianus Rufus, who was one of the duumvirs, built the theatre at his own expence. There are numbers of other inscriptions, some perfect, others imperfect ; which latter is owing, I suppose, to the little care taken in digging them out.

It is a great pity, that they did not, at the first discovery, open the ground at the top, and clear it away as they worked, in order to have seen those fine things in open day-light. But I have been told, it was impossible, seeing the vast depth of earth and stone they must have been obliged to have made way thro'. That reason does not all satisfy me ; they having slaves enough, of the rascally and villainous sort, to complete such a work. What a fine thing would it have been to have come directly down to the roof of the building, instead of digging round, and to have found all things in their first situation !

I come

I come now to mention another opening, distant from that, which leads to the theatre, by which they have made a way into some houses. Here they seem to have dug infinitely more than about the theatre; for one may ramble, as in a labyrinth, for, at least, half a mile. I cannot be very particular in describing the many things, that have been dug out of either of the two places; but the most noted you shall have, as far as my memory will permit, besides those, which I took down in my pocket-book on the spot. The first were many parts of broken horses, with part of a triumphal car or chariot, all of gilt bronze; and which, they say, was placed over one of the gates of the theatre.

The next were two equestrian statues, which were found on each side of one of the said gates, and, they suppose, fronting a street, that led to the theatre. Those, I was told, were erected in honour of the two Balbi's, father and son, as having been great benefactors to the Herculaneans. One of these statues is so broken, that it cannot be repair'd; the other, which happen'd to be better preserved, is extremely well repair'd, and is set up under the piazza in the gate-way of the king's palace at Portici. On the front of the pedestal is seen the following inscription, as it was found;

M.NOMIOMF  
BALBO  
PR. PROCOS  
HERCVLANENSES.

It is certainly a most beautiful statue, and is judg'd, by all connoisseurs, to be one of the best in the world. I must own, I never saw so much life express'd in any figure I have seen. Not far from it, at the bottom of the palace stair-case, is fixed another beautiful statue of the emperor Vitellius, very perfect and intire: another statue of Nero, with a thunderbolt in his hand: another of Vespasian: one of Claudius: one of Germanicus: two other beautiful statues, sitting; but I could not learn whom they represented. There are many others, some of marble, some of bronze, all bigger than life; and even some gigantic, or colossial: many without heads, or arms, and others so destroy'd, as never to be repair'd. Of busto's there are several; some very beautiful, as that of Jupiter Ammon, Neptune, Mercury, Juno, Ceres, Pallas, &c. In the apartments of the palace are a vast number of little statues, many of which are extremely beautiful: also a great number of little idols, tripods, lachrymatories, and many vases curiously wrought. Among these is a whole loaf of bread burnt to a coal: they will not suffer any one to touch it. It is cover'd with a glass bell, thro' which I perceived letters on the loaf, which possibly were the baker's mark; and, examining them with attention, they stood thus;

S | ILIGO. CRANII  
E CISER

The man, who shew'd the curiosities, told us, that several had attempted to explain this mark, but could  
not



not make it out ; which, I believe, was owing to the first word. The S, they imagined, signified a word by itself ; as it stands a little wide from the letters, that follow, and to which I judge that S ought to be joined to form SILIGO, which signifies fine flour ; of which the bread might be made, with the mixture of flour of chich-pease or vetch, which I think ECISER signifies. CRANII I take to be the baker's name. Whether I am right or not, I cannot say ; but the man seem'd to approve of the explanation, and said he would communicate it to the king. There are many other valuable curiosities, which I could not see, being lock'd up in the king's closet, and private apartments ; such as medals, intaglias, and cameo's.

I come now to mention some of the pictures, which were found. Some of them were taken out of a temple near the theatre, others from the houses. They have all preserv'd their colours to admiration ; which are very lively. They are painted in fresco, and were sawed out of the walls, not without a great deal of trouble and care ; and are now fixed, with binding mortar, or cement, in shallow wooden cases, to prevent their breaking, and varnish'd over, to preserve their colours. You must think, that these pictures are not alike valuable, otherwise than from their antiquity ; some doubtless having been done by good hands, others by bad, as one sees by the works of those now-a-days. I shall therefore only mention some of the best. There are two large ones, as big as life, which were taken out of the temple, which I mention'd, and which, as the antiquarians will have it, was dedicated to Bacchus ; proving it by some  
other

other particular things found in the said temple. One of these pictures, they say, represents Theseus. The figure is naked, and holds a small club in his hand: between his legs lies a minotaur, the posture of which produces one of the most admirable foreshortenings, that ever I beheld. There stand about him also three little boys, one of which kisses his right hand, another embraces his left arm, and the third gently embraces his left hand; all extremely well expressed. The other picture is of the same size as the former, and composed of many figures as large as the life. 1. A woman sitting with a wand in her hand, and crown'd with flowers: on one side of her stands a basket of pomegranates, grapes, and other fruit: near her is a little satyr or fawn, playing on one of the ancient instruments, of 6 or 8 tubes, joined together in a row. There is a lusty naked man standing by her, with his face turned something towards her, with a short black beard. He has a bow and quiver of arrows; also a club. In the same piece is also another woman, who seemingly is talking to the first: she is crown'd with ears of corn. There is also a hind giving suck to a boy. The man told me, that this represented the story of the discovery of Telephus. Another picture represents a winged Mercury, with a child sitting on his shoulders across his neck, by whom is a woman sitting, and taking Mercury by the hand. This, we were told, was supposed to be Bacchus carried to nurse. Another piece represented Jupiter embracing Ganymede. Another, in which is a hunt of stags and swans. Three others, in each a Medusa's head. Another, representing two heads of imaginary animals; for never was the like on  
S earth,

earth, nor in the waters under the earth. Another beautiful one, representing two of the muses, one playing on the lyre, the other with a mask on her head. Another, with a lion, wood, and distant views. In another, various centaurs, buildings, &c. In another, a stag; over which is a bird flying, and seeming to beak at him. Two other small pictures of a dolphin. Another with architecture, and distant views. One with a peacock. Another with a temple, adorn'd with various pillars.

There are many others of less notice, which you will give me leave to omit, that I may mention other things, which have been found; namely, two large cornucopia's of bronze gilt; a large round shield of metal; two metal dishes; several lachrymatories of glass, others of earth; four large candlesticks of bronze; a large metal vase with a handle; many others of earth, curiously wrought: the foot of a lion most curious, but in marble, and which supported a marble table; a beautiful mascharron of metal, having the face of a cat, with a mouse in her mouth. There is also a very fine medallion, extremely well preserved, with a basso-relievo on both sides; on one is a woman, by whom is a man naked, killing a hog: on the reverse, is an old man, naked to his waist, sitting and playing on two pipes, which he holds in his hands. There is also another odd piece in basso-relievo, which represents a green parrot, drawn in a chariot, and driven by a green grasshopper, which sits on the box, as coachman. Whether this alluded to any thing, I could not learn; but I rather imagine it to be a whim of the artist.

Were I to recount all the things in particular, that I saw, it would fill up more paper by twenty times,  
than

than I have already scribbled. Let it suffice then, that there are many baskets and cases full of one thing and another, all jumbled together; such as kitchen utensils, locks, bolts, rings, hinges, and all of brass. Things, that were of iron, were totally eaten up with rust. I was told, that when the workmen came to any thing of that sort, it moulder'd to dust as soon as they touch'd it; occasion'd doubtless by the dampness of the earth, and the many ages it lay buried. I have little more to mention about the curiosities; I will only tell you, that I was assured there were found many vases, and chrystal bottles full of water; but that might penetrate thro' the earth, and fall into them, if not close stopp'd: also a sort of standish, or inkhorn, in which were found many stylets or pens, with which they wrote in those days. When it was first taken out, they say the ink had not only its natural colour, but that it was yet capable of tinging: it was very dry, when I touch'd it. There were eggs found quite whole, but empty; also nuts and almonds; grain of several sorts, beans and pease. I have by me some of it, which resembles beans of the size of coffee-berries burnt quite black. Many other sorts of fruit were found burnt quite to a coal, tho' otherwise whole and intire.

I will close this narrative, by declaring, that I cannot be of the sentiments of some, who assert, that this city was suddenly swallow'd up, which implies, that the earth must have open'd, and formed a pit to receive it. My opinion is, that it was overwhelmed with the boiling matter issuing from the mountain, at the time of the eruption. My reason for this conjecture is, that most things were found upright,

chiefly the buildings. That it was not a sudden overwhelming, and that the inhabitants had time to escape with their lives, tho' not with their goods, is proved, by their not finding dead bodies, where they have hitherto dug. It is said, some human bones were found, tho' few; which perhaps might belong to some miserable bedridden wretch or other, who could not escape, or of a person dying suddenly thro' fright; which I think is not difficult to imagine, when one considers what a scene of horror they must have had before their eyes.

Very little money or plate has been found, or any other portable thing of great value; which I think is another proof, that the inhabitants were not destroy'd. I doubt not, but before the violent eruption came on, the people for some days might perceive such tokens and signs, as could not but alarm them, and put them on their guard.

At the eruption, which happen'd in 1737, before it burst forth for some days, the inhabitants of Portici, and the adjacent villages, all retired; being by some signs apprised of the event. And I have been assured, that even for seven years before this last eruption, they were under daily apprehensions of it; but more so for the last four months of that time, as the mountain then scarce voided any smoke at all, and continual rumblings were heard from the body of the mountain, even at a great distance. The torrent of burning matter at this eruption took its course the opposite way from Portici or Herculaneum, and, as it happen'd, no village was damaged thereby. A convent of Carmelite friars, that stood in its passage, had a share of it; but what it most destroyed were  
corn-fields,

corn-fields, vineyards, and some woodlands, upon which the matter lay to a great thickness, and they say retain'd its heat for a long while. I was inform'd even by several, who had been on the spot 4 months after the eruption, that the matter (which they call the lava) was yet so hot, that they could not walk upon it: which shews it must be of a prodigious depth or thickness. This matter, it seems, is not of the same quality nor substance all the way thro' the body of it; for I observ'd, when I went to the theatre, as I descend'd, that the sides of the passage at the entrance were a sort of mould, 8 or 10 feet thick; after which appear'd stone, of a blackish or dark-grey colour, to the thickness of about a yard or 4 feet; then another layer of sandy earth, under which was a layer of the same sort of stone; and that it continued *stratum super stratum*, till I got to the bottom. The theatre and the houses seem all to have been fill'd with earth, and being heavier at that instant than any other part, of which the matter was compounded, subsided first. I know a cubic block of stone is heavier than a lump of any earth of the same dimensions. Therefore you may imagine, that the stony part of the matter should precipitate first; but my notion is, that when this stony matter was liquified, and boiling with heat, it was lighter in proportion than the earthy part; and that the instant the boiling degree of heat was over, it could not gather its parts together quick enough to form a compact heavy body, before the earthy part subsided. I have examin'd this stone, and find it has not, everywhere, the same solidity. Reasons might be given for that, but I will not trouble you with them now:  
I will

I will only tell you, that, in general, this stone is very hard and heavy, and that the whole city of Naples is paved with it. I have seen some of it, that will bear a fine polish, and of which they make snuff-boxes.

XIX. *A Letter to the President, concerning the Hermaphrodite shewn in London: by James Parsons M. D. F. R. S.*

S I R, Red-lion square, March 14, 1750.

Read March 14.  
1750.

**A**S I find the French girl, now shewn at Ludgate as an hermaphrodite, makes some noise in town, and as the generality of the world are apt in this very case to take the erroneous side of the question in giving their opinions about it, for want of having a proper knowledge of the parts, I have taken the liberty to trouble you with this letter, containing some account of the matter, which is intended to undeceive such as are mistaken about it.

She is now about eighteen years old, and the true description of her *pudenda* is as follows:

What is mistaken for a *penis* and has at first sight caused the deception, is the *clitoris*, grown to an inordinate size. The prepuce of this is continued down on each side, to form the *nymphæ*: under these the natural *urethra* is in its proper place, as in all females; and just under this is a natural *vagina*. This *vagina* is concealed by a skin growing up from the *perineum*, and continued to the *labium* of each side

hide quite over it; which, if snipp'd with scissars, would lay the orifice of the *vagina* bare, and shew the person a perfect female, having only this morbid size of the *clitoris*.

This is really the fact relating to the present subject; which any one may be satisfied of, by passing a finger down under this skin to the *perinæum*, and he will meet the orifice of the *vagina*, and find it as perfect as that of any other woman of the same age.

The *vagina* being thus cover'd, and the *clitoris* thus large, it is no great wonder, that she should at first sight be taken for a male by the vulgar: but it would seem a little too careless in any of the faculty to be so deceiv'd. However if we do but consider the following observations, we shall find it no such strange affair, as it now seems to the world: nor is it new, to find people imagine, that, since this mistaken *penis* is imperforate, the *urethra* is preternaturally directed to appear under it, without considering it to be a true female *urethra*, in its natural place.

I had the honour, on the 30th of April, 1741, to lay before the Society seven or eight female *fætus's*, from about six to somewhat more than seven months growth. Each of these had its *clitoris* bigger in proportion than the present girl, or any other whom I have ever seen; which is the case with all female *fætus's*, during the greatest part of the time of gestation. And this is nature's common rule all over the world.

Now it is impossible, that so many hermaphrodites should be formed at once, since we have so few instances



instances among the European nations of those so reputed ; tho' they are common enough in Asia and Africa, in all those places especially, that are nearest the equinoctial line, where the non naturals themselves conduce much to the general relaxation of the solids in human bodies, and consequently this unseemly accretion of that part.

Now as the female *fœtus* increases in the *uterus* in a natural way, the neighbouring parts of the *pudenda* grow more in proportion than the *clitoris*, drawing away the integuments from it, whereby it becomes by degrees less conspicuous ; and at length, as the child grows up, it is shrunk between the *labia*, and remains always cover'd, as it is now the common appearance in our women. But when it continues its growth, together with the neighbouring parts in the same proportion, which all female *fœtus's* have it in, maintaining its first proportional size, the person, when grown up, is call'd by the vulgar an hermaphrodite ; since the natural structure of this part is in a great measure like that of a *penis virilis*.

Nor is its largeness in a *fœtus* much to be wonder'd at, since there are other very similar cases in the same body, as the *gland thymus* and *glandulæ renales* ; both which, as the child grows large, diminish in their proportion.

These *macroclitoridæ* are so numerous among many nations of *Asia* and *Africa*, that the antients, Albucasis especially in his 71st chap. inform us of the necessary operation and method of cure, which he terms *cura tentiginis*, finding the part so call'd inconvenient from its largeness. Nor was this knowledge confin'd to men of science alone amongst the *Egyptians* and *Ethiopians*,

*Ethiopians*, and *Angolans*; for every parent knows, when the child has these parts longer than ordinary, and cut or burn them off, while girls are very young, and at the same time never entertain the least notion of the existence of any other nature besides the true female, in those children, who are thus deprived of that part.

The learned *De Graafe* was well acquainted with this, and gives his approbation of the operation, as highly necessary, as well as decent: “*estque hujus partis chirurgia orientalibus tam necessaria quam decora.*”

It has been said too, that this girl in town has not the least appearance of breasts; but those, who report this, must surely have never seen the breasts of the women of any other nation but our own. On the contrary, she has as large breasts as any French girl of her age, and as good a nipple; whatever care they take to squeeze and compress them with her apparel. Besides she is a thin girl, and small of her age; and really among our own young women, when they are spare, and small in stature, it will be hard to find any with breasts more conspicuous than, if so much as, hers.

I have consider'd this subject more at large in my *Critical Inquiry into the Nature of Hermaphrodites*, which the curious may see; and am, in the mean time, Sir,

Your most humble servant,

J. Parsons.

XX. *An Account of a very small Monkey, communicated to Martin Folkes Esq; LL. D. and President of the Royal and Antiquarian Societies, London; by James Parsons M. D. F. R. S.*

S I R,

Read April 18. 1751. **T**HE right honourable the Lord Kingston, of Queen's Square, permitted me to take a drawing and this description of the little monkey, which you and the reverend Dr. Stukeley saw a few days ago. Its particular characters, join'd to its very small size, induced me to think it a subject worthy the notice of the Royal Society; especially too as there is yet no good figure of it exhibited.

It is, from the tip of the nose to the root of the tail upon the edge of the spine, but seven inches and an half; and the tail, from its root to the extremity, is nine inches; its face about an inch long; and hardly three quarters of an inch broad at the eyes, where it is broadest. Its utmost weight is about four ounces and an half averdupoise.

The face is naked, and of a flesh-colour; the eyes black, having no white part visible; the ears are thin, large in proportion, and of a dark colour; and are surrounded each with a grove of very white hairs; between which the hairs of the neck are blackish, and so are the four extremities: the rest of the body and tail is a mixture of dusky and yellow, so as to compose a dark olive; the hairs of the body are









exceedingly soft, and, upon closely examining them, each hair is parti-colour'd, that is, dusky at the root, then a little yellowish, then dark, and then yellowish again, somewhat like the soft feathers of partridges. The fingers are slender, each having three joints: they are five on each extremity, and are pointed by nails rather resembling the claws of birds, than those of human bodies; which is common to most other species of the *cercopitheci*.

I since waited on Mr. Hyde, of Charterhouse-square, who shew'd me another of these, which happens to be the male of this very species now described, and seems about one size larger than my lord's, being about eight inches, measured by a pack-thread, from the nose to the root of the tail, and from thence, the tail is about ten inches long. It weighs about six ounces and a quarter, is very slender like the female, and with some difficulty moves his posterior extremities; but they seem always better in warm weather, and more active than in winter, being scarce able to bear cold.

The same gentleman gave me besides an account of the following particulars relating to it:

This and a female, which is since dead, were brought by an East-India ship about two years ago, from Brasile, having occasionally touch'd there in its return from the Indies: which shews Brasile to be the native place of these animals. And as Mr. Hyde has had his monkey now two years, these may be reasonably supposed at their full growth; and perhaps the males are commonly somewhat larger than the females, as it is in some other animals. They are both very thin and spare, and of the same colour in every respect,



except that there are more downy white hairs on the male than on the female; the marks and features are the same in both; their voices are small and shrill; and they are alike in all other respects but the sex; and altho' the claws are like those of a bird having hooked nails, pointed at the end of every finger of the upper extremities, the thumb of each inferior extremity in male and female is flat like the human, and has a flat nail.

Mr. Hyde feeds his monkey sometimes with roasted chesnuts, sometimes in summer with sweet fruits, as gooseberries when thorough ripe, plums, cherries, and such-like; but he will not touch currants, because of their acidity. He seems very fond of the smaller spiders and their eggs; but not the larger sorts; nor will he touch the great blue-bottle fly, tho' he greedily eats the small common flies. He frequently has a diarrhoea; and once, by accident, it was found, that he seem'd to love a gum, call'd gum-senega, which he feeds on with eagerness, and it never fails to cure him; so that he gives it to him now only occasionally. Another kind of diet, which I saw him eat, was young snails; of which he eats three a day.

I have chosen the figure of the male to be engraved for the *Transactions*, put into such an attitude, as will best shew the *penis* and *scrotum*. They are placed nearly in the same situation with those of a dog; but most resemble those of human nature, being naked of hair, having a fair soft flesh-colour'd skin, very tender and taper towards the end of the *penis*, which is altogether as prominent from the body as the human. The feminine part of generation of this species  
consists

consists of a flesh-colour'd naked piece a little raised, having a hole in the middle, and situated backwards between the *femora*, not quite so far as the *judenda* of the females of other small quadrupeds.

This, sir, is the species of monkey mention'd by Marcgravius, in the fifth chapter of the sixth book of his *Historia rerum naturalium*, &c. where he treats of the quadrupeds and serpents of Brasile; but his figure bears so little resemblance to the creature, and his description is so short, that I believe you will think this farther history not unnecessary: however, I have transcribed his words as follow, to shew that this animal is the same, that he describes.

“ Cagui or sagui minor, tenerum animalculum et  
 “ parvum, leonem quoque facie referens. Totius corporis  
 “ longitudo circiter sex digitorum est, caudæ autem  
 “ decem; capitulum habet parvulum, quod vix pomum minus æquat: nasum exiguum elatum; oculos teneros; os parvum cum dentibus acutissimis;  
 “ crura manus habent instar cercopithecorum, quinque  
 “ digitis teneris prædita: aures subrotundas, quas  
 “ circumstant pili albi, ordine et cumulatim positi  
 “ et quasi eleganter essent pexi. Pili autem totius  
 “ corporis interius et in exortu rufescunt, exterius  
 “ sunt ex albo et fusco mixti; cauda autem quasi  
 “ ex albo et fusco annulata est. Acutissimum edit  
 “ sonum voce sua: velocissimum est animalculum in  
 “ saliendo: frigoris impatientissimum. Vescitur pane,  
 “ farina mandiocæ, atque aliis.”

What the Brasilians call *cagui*, the Congenses call *pongi*; which are distinguished into the *cagui major* and

and *minor*. Mr. Ray, in his *Synopsis anim.* p. 154, chapter of monkeys, thinks, this is that species described by Clusius from Lælius, which they call *Sagouin*. I am,

S I R,

Your truly humble servant,

James Parsons.

XXI. *Extract of a Letter from Naples, concerning Herculaneum, containing an Account and Description of the Place, and what has been found in it.*

Read April 18. <sup>1751.</sup> **T**HE entrance into Herculaneum is described to be down a narrow passage, cut with a gradual descent; and, towards the bottom, into steps: and the city is supposed to lie about 60 feet under the surface of the ground. Those, who go down into it, carry each of them a wax taper, and are preceded by a guide. It is supposed, that, besides the earthquake, which swallow'd up this town, it was also at the same time overwhelmed with the burning lava, which then ran down from mount Vesuvius, during the eruption. And accordingly all the passages into it are cut thro' this lava; which is a very hard substance, like stone, of a slate-colour, and said to be composed of various kinds of metals and glass; which indeed is manifest in the appearance of it. The streets of Naples are paved with the same lava: but it seems to be of a  
much

much more soft and sandy substance in Herculaneum, than in the places, where they dig it for use.

The appearance of this city would greatly disappoint such, as should have raised their expectation to see in it spacious streets and fronts of houses ; for they would find nothing but long narrow passages, just high enough to walk upright in, with a basket upon the head ; and wide enough for the workmen, who carry them, to pass each other, with the dirt they dig out. There is a vast number of these passages, cut one out of another ; so that one might perhaps walk the space of two miles, by going up every turning.

Their method of digging is this. Whenever they find a wall, they clear a passage along the side of it. When they come to an angle, they turn with it ; and when they come to a door or a window, they make their way into it. But when they have so done, they are far from finding themselves in a spacious room, or open area ; for all the rooms and places they have yet found, are filled so brimfull with lava, that it sticks on to the sides of the walls ; and they can advance no farther, than as they can make their way by digging : which is such infinite labour, that when they cease to find any thing worth their search, they fill up the place again, and begin to dig elsewhere. By which means no place is quite cleared, to the great grief of every one, who has the least share of curiosity. But the king does not chuse to proceed in any other method. Consequently, it does not appear how many stories high the houses may be ; nor is any thing to be seen over the head but lava. In which lava are vast numbers of burnt beams, that  
seem

seem to have been beams or joists of floors ; tho' they are now little more than black dust ; and where they are quite moulder'd away, one may plainly see the grain of the wood imprinted in the lava ; so close did it stick.

In one passage, they passed by a great many pillars, lying about three feet distant from each other ; supposed to have composed a portico, or colonnade. They are of brick, plaster'd, and are fluted, and painted red. They are broken off, a little above the base, and are thrown down, in such a manner, that they now lie in an horizon al position, in the midst of the lava.

In another place, they passed through a sepulchre, a little kind of room, about 12 feet square ; which was built up, all round, in the same manner as stoves are in our modern kitchens, with niches, like the arched holes under such stoves, for the ashes to fall into. In each of those niches was a common earthen urn or pot, with a cover, full of dry bones, appearing as if they were worm-eaten.

In another part, they manifestly went in at the door of an house ; and saw a window a little on one side of it. They seemed to be in a good large room ; but the lava was left all standing in the middle of it, and only a passage made round it, in order to get the paintings off from the walls. There have been several rooms opened, from whence they have taken away paintings and mosaic floors, but which are now filled up again. Some bits of mosaic floors still remain, and are visible.

They passed another place, which is called a bath, and has that appearance. It is of a circular form,  
and

and seems to have been made to contain water. Here were found some marble and some statues. And a little way distant from this is a stone stair-case; but what it leads to, is not yet known.

Then they passed by a well, built round with a parapet-wall on the top, and an arch turned over it; whereby the lava has been prevented from choaking it up; and it is now a good well.

In another place, they walked, for about 30 feet, in a strait line, along the side of a stone building, supposed to be a temple. It has two very deep steps all along the bottom; and then an upright flat surface, about 4 or 5 feet high; and then a narrow cornice; and seems to be the basis or pedestal for a colonnade of pillars. In one part of it they have begun to dig, above the cornice, and find no interruption; which adds to the probability of its being the space between the pillars. However, none are yet discovered; and it will be some time, before they can be able to determine what it is.

In another place is just such another building of the like sort but of a circular form. This they have but just begun to find.

In some places the company saw little bits of paintings on the walls; but they are taken away presently after they are found.

It is supposed, that the workmen are at present got no farther than the suburbs of the town, in this part of their search; having met with no grand buildings, unless the two last-mention'd should prove to be such.

But the theatre (which is mention'd by the writer of the letter as most worth seeing) is about  
U half

half a quarter of a mile from the place, where the company first went down. The company therefore now re-ascended, and walked thither.

A very good view may be had of this theatre, even without descending under-ground ; for, over the seats, a very large well is dug through the ground, and through the lava ; the diameter of which well may be perhaps 15 or 20 feet ; and the depth of it about 60 feet ; and the sides of it are all smoothed and white-washed : so that it lets in a very strong light ; and a person may look down from the top, and have a very good view of the seats : but no one can see the whole of it, without going down under-ground, which this party therefore did.

They perceived, that a passage had been cleared all round the outside wall of it ; which appears to have been plaistered, and painted with pillars, and other kinds of ornaments ; most of which are taken away. They walked all round the corridor on the inside, which led to the seats. It is here totally cleared of the lava ; and they could see the arched roof, which is plaister'd. This corridor was lined and paved with marble ; but it is now all taken away. There are 25 rows of seats, all of stone. There is a pretty wide space of them, cleared quite down to the bottom ; so that a very perfect view may be had of them. The door-ways are also all cleared ; and likewise the little stair-cases, 8 or 10 in number, which led to these seats. But part of the *arena* remains not yet cleared. This whole building seems to be perfectly intire ; and nothing appears to be thrown out of its place. It is imagined, that it served both for a theatre and an amphitheatre. There does not  
appear

appear to have been any covering over the seats. It was in the niches of the corridor of it, that almost all the fine statues were found.

The writer of the letter observes, that “ the notion “ of this theatre’s being full, when the eruption happened, and that the people had not time to “ escape,” was probably groundless; because no dead bodies have been found in it. To which is added another reason for judging, that the destruction of the city was not absolutely sudden; which is, the small quantity of riches hitherto found in it; as well as the very small number of bodies and bones, not amounting, in all, to above 20 skeletons, if so many. And one very extraordinary instance is alleged in support of this opinion, “ that they had at least *some* “ notice;” however short it might be. A skeleton was found in a door-way, in a running attitude; with one arm extended, which appeared to have had a bag of money in the hand of it: for the lava had taken so exact an impression of the man, that there was a hole under the hand of the extended arm; which hole was apparently the impression of the bag, and several pieces of silver coin were found in it. This man therefore must have had notice enough of the danger, to endeavour to secure his treasure; tho’ he must have been, as is remark’d, instantaneously encompassed with liquid fire, in attempting it.

No manuscripts have yet been found; but they have met with some few inscriptions on marble, tho’ none, that are of any consequence, or serve to give new light in any point of antiquity.



The labour of clearing the place is performed by slaves, who work chained together, two and two.

The curiosities taken out of it are deposited at a palace of the king's, at Portici; and fill several rooms there.

The finest of them are the statues. There is an exceedingly beautiful one, in white marble, of Balbus, on horseback; which stands in a portico of the palace, and is a most justly admired performance. It is quite intire; and the horse is reckoned the finest piece of work of that kind. The other statues are not yet put up. There are many of them; some in marble, some in bronze, and almost all of them fine. Particularly, one of Agrippina; also a figure of a woman, with a dejected countenance, which is the most expressive of sorrow, innocence, modesty, and diffidence, that it is possible to conceive. Some of the bronze statues are remarkable for having a sort of enamelled eyes put into them; but the whites of them look very shocking.

The marble, that has been found, is very fine, and of various sorts; and the king has made most beautiful tables of it.

The writer proceeds next to give some account of the paintings, and observes, that, to speak the truth, much the greatest part of them are but a very few degrees better than what you will see upon an ale-house-wall. They are all painted on plaister; which has been very carefully separated from the wall, in as large pieces as might be done. These pieces are now framed; and there are above 1500 of them, but not above 20, that are tolerable. The best of them are 3 large pieces; one of which is a sort of history-

history-piece, containing 4 figures, that have some expression in their faces; but even these best, if they were modern performances, would hardly be thought worthy of a place in a garret. There are about a dozen little pieces, of women dancing, centaurs, &c. the attitudes of which are very genteel, and the drawing pretty; but the shading is terrible daubing.

The colouring, that has been so much talk'd of, is allow'd to be surprisingly fresh, and well preserv'd, considering how long it has been done; but the painters of them seem to have been masters of only a very few simple colours, and those not very good ones. The red is the brightest and best. The lava was found sticking on to all the painting; which, as some think, has helped to preserve it. The paint is liable to be rubbed off; to prevent which inconvenience, they have slightly varnished it.

The designs of the greatest part of these paintings are so strange and uncouth, that it is difficult, and almost impossible, to guess what was aimed at. A vast deal of it looks like such Chinese borders and ornaments, as we see painted upon screens. There are great numbers of little figures, dancing upon ropes; some few small bad landscapes; and some very odd pieces, either emblematical, or perhaps only the painter's whim. Of which last the writer gives two specimens; one, of a grasshopper driving a parrot; the other, of a vast great head, in the midst of what seems to have been intended for a green field encompassed with an hedge.

All the paintings are either upon black or red grounds: and such, that the writer cannot help suspecting, that it is their antiquity alone, that has recom-  
mended

mended them to their admirers, and atoned, in their eyes, for all their blemishes and defects; and professes great amazement at the accounts, which have been sent to England concerning them.

Then follows a little sort of inventory of things found in this subterraneous town; kitchen-furniture, in abundance, in iron and in copper; apparently answering the same purposes, for which we now use them, tho' a little different in shape; vast numbers of lamps, both earthen and copper; locks, hinges, &c. A loaf of bread, almost burnt to a coal, with the baker's name upon it. Some beans and barley. A fishing-net, burnt quite black; but yet hanging together, so that one may plainly see the meshes, and what the thing has been. Some urns and tripods, in bronze, chased in a very neat and curious manner; the chasing in silver. Some busts. A good many small figures, and medals; but the king is so choice of these last, that they are not to be seen, tho' said to be not very curious. All the coin, which they have found, has been silver. There are a few good *intaglio's* and *cameo's*. There is a pair of bracelets, which were found on the wrist of a skeleton; also a few ear-rings, and some rings.

The king has laid down, in the rooms at Portici, several of the mosaic pavements, that were found at Herculaneum. The designs of them are pretty enough, but not uncommon. They are, chiefly, black and white marble; and very small squares. They are laid in a cement, but so clumsily, that the pieces do not touch at all; and the same thing was observ'd at Herculaneum.

The

The king is now employing a person to take drawings of all the statues, and principal paintings; with an intent to publish them, together with an account of Herculaneum. The statues cannot be made to appear more beautiful than they really are: but the writer imagines the world will be vastly deceived with regard to the paintings. For the man is a very nice drawer; and has also managed the colouring to advantage; so that he has made exceedingly pretty things, from originals, which are miserable daubings. The company having seen the drawings first, were extremely disappointed, when they afterwards came to view the originals. It is likewise proposed to make a plan of the town, by measuring all the walls, which they find, and taking all the angles; and thus, in some degree, to compensate for the omission of laying it all open.

XXII. *An Occultation of the Planet Venus by the Moon in the Day-time, observed in Surrey-street, London, April 16, 1751, O. St. by Dr. John Bevis.*

Read April 18. 1751. **F**INDING many had gotten a notion from the almanac-makers, that it would be next to impossible to observe this occultation, I was resolved to give attention to it; well remembering, that I had several times seen Venus on the meridian with a three-foot transitory, when she was much nearer her superior conjunction with the sun, than now. The whole matter was to direct a  
tube

tube so, as to find her out a little before her ingress, and to manage the instrument so, as also to have sight of her at the instant of her egress. And knowing, that Mr. Short is never unprovided with one or more instruments exceedingly well adapted to this and other purposes, the same that he has described in *Phil. Transf.* N<sup>o</sup> 493; which, for its easy removal from place to place, may be consider'd as a sort of portable observatory, I intimated my intention to him the evening before; who was so kind as to set up two of the said instruments, which I found rectified, and ready for observation, when I visited him the next morning.

One of these, placed near his clock, he intended for his own use, and the other was for me. I had also with me a watch of Mr. Graham's make, which shewed seconds, and was set exactly to the clock.

A little after 10 Mr. Short waited upon His Grace the Duke of Queensbury, and Mr. Pringle, to the apartment where I was; who, after taking a look at Venus, which I had then brought into the telescope, seated themselves near me, and I applied myself attentively to the observation.

The air was of itself exceedingly clear; but the wind, being in the north-east quarter, brought such drifts of smোক, as much impaired the distinctness of Venus, which however look'd round. Several minutes before I expected it, the figure of the planet was manifestly alter'd; upon which I called out to Mr. Short to hasten to his instrument, which he did, but was too late. I never stirred my eye from mine, before the total ingress, at 10<sup>h</sup> 39' 30'' by the watch,  
which

which I compared with the clock, and found it had not altered in the least.

From my first perceiving the change of the figure, to the intire ingrefs, could not be a full minute.

By a flight *calculus* I had made, the occultation was not to last half an hour; but the ingrefs considerably anticipating it, I conjectured, that, on the contrary, the egress would be later, as it proved to be,

I must here take notice, that not the least glimpse of the moon, then not two days old, could be discerned: so that the business of securing Venus, at the instant of her emersion, within the field of the telescope, over which she passed in about  $2^{\circ} 10''$ , depended intirely on a due management of the screw, which gave motion both to the equatorial or horary plate, and to the telescope. A little after 11 I brought the point of the hour-circle, answering to Venus, to the index, and might then have seen her near the middle of the field, had she already emerged. Every two minutes after I was careful to turn the screw so much, as to be sure of keeping her within the field. At length clapping my eye to the instrument immediately after one of these operations, I perceived her quite emerged and round: this was at  $11^{\text{h}} 13' 15''$  by the watch, which still kept exact pace with the clock.

I cannot think my eye had been removed more than a minute: my Lord Duke judged not quite so much.

Mr. Short had the misfortune not to recover sight of Venus till about a minute later than I did, for want of an assistant, who knew how to govern the screw.

Venus passed the meridian in the transitory a  
 $1^h 37' 55''$  afternoon by the clock: the sun pass'd  
 this day at  $11^h 57' 27''$ ; and yesterday, the 15, at  
 $11^h 57' 28\frac{1}{2}''$ ; whence it is easy to reduce all to ap-  
 parent time, as follows:

Total ingrefs of Venus	1751, Apr. 15	22	42	02
Her total emersion	—	—	23	15 47
Her meridian tranfit	—	16	01	40 29

Now, supposing the whole disk to  
 have taken up one minute, as it  
 seem'd thereabout, both in the ingrefs  
 and egress, the middle of the occu-  
 lation must have been ————— 15 22 58 24 $\frac{1}{2}$

And the duration, with respect to  
 the centre of Venus ————— 33 45

In this account I have been the more particular  
 as to circumstances, in hopes to point out, in some  
 measure, to such, as may not be much conversant  
 in observation, how to provide, and what to do,  
 on a like occasion; but more especially to recom-  
 mend the more frequent use of the polar axis; the  
 great conveniency whereof I have frequently expe-  
 rienced, not only in readily finding and easily pur-  
 suing a celestial object, by day as well as by night,  
 but in many other regards, as in comparing un-  
 known phenomena, as comets, &c. with known  
 ones, in any situation, only by the addition of a  
 graduated sector; according to Mr. Graham's excel-  
 lent contrivance; in measuring diameters, and repeat-  
 ing the mensuration, as fast as you please, with the  
 micrometer; which, in this way of application,  
 admits

admits of a far simpler construction than in any other. Add to these the very easy, but otherwise impossible, management of the most heavy and cumbersome instruments, such as the sector, which the late Mr. Flamsteed made use of for measuring angular distances at Greenwich.

When the great reflecting telescope, that is set up at Marlborough-house, was nigh finished, it was proposed to support and direct it by means of a complicated machinery, intirely different from the apparatus, which is now applied to it. This I strongly opposed in behalf of a polar axis, which was at last agreed upon; and as soon as it was executed, it appeared, to the full satisfaction of the generous owner, and the curious artist, that so vast a weight as more than one thousand pounds could be moved and directed at pleasure, even by a stranger, with a finger and a thumb.

J. Bevis.

Read April 18. 1751. **I** AM informed by Mr. John Canton, that he observed the occultation of Venus by the moon last Tuesday, at his house in Spital-square, and found the immersion at 10<sup>h</sup> 42' 20" *a.m.*  
emersion at 11 15 40

April 18, 1751.

J. Short.



XXIII. *An Account of a remarkable Appearance in the Moon, April 22, 1751, by James Short, F. R. S.*

Read April 25. 1751. **I**N Numb. 396 of the *Philosoph. Trans.* there is an account of an observation made on a particular and uncommon appearance of the lunar spot called *Plato* in the nomenclature of Riccioli's and Grimaldi's *Selenography*, and *Lacus niger major* in that of Hevelius. Signor Bianchini, to whom we owe this communication, says, that it was the 16 of August 1725, N. St. about an hour after sun-set, when he took his observation with a dioptric telescope, of the length of 150 Roman palms (about 110 English feet) made by the famous Campani, the air being very serene, and the moon (as he says, speaking of the same phænomenon in his book of *Venus*) a day past the first quarter: so that the said spot then lay in the common section of light and darkness. The mountainous oval margin, with which it is surrounded, was brightly illumin'd with the sun's rays; but the plain bottom look'd darkish as having not yet received his light. There was however extended along its area, from end to end, a track of reddish light, as though a beam had been admitted through some perforation in that side of the margin, which was then exposed to the sun. M. Bianchini proposes the solution of this matter in two different ways: first, by supposing an aperture in the margin, as just now mentioned: or, secondly, by conceiving the moon to have an atmosphere, and that  
thereby

thereby the rays passing near the summit of the margin might be so refracted, as to be thrown upon the plain area or bottom.

Having lately had an opportunity of observing something of the same nature myself, I take the liberty to lay it before the Society: as also to entreat their opinion about my conjecture concerning the cause thereof.

Monday, April 22, 1751, O. St. being at Marlborough-house along with Dr. Stephens and Mr. Harris, and having directed the great reflector to the moon, I perceived a single streak of light projected along the flat bottom of the spot *Plato*; and from what I was then able to recollect of Signor Bianchini's narrative, I could make no doubt but that it was of the same kind with that, which he saw, and which I had so often looked after in vain. By the position of the spot on the disk, and the shadow of the mountains on the west side of it, we should not have expected to have seen any light on the bottom. Soon after we discerned another streak of light extended along the bottom, parallel to the first, but somewhat lower, which in a very short time was evidently divided into two. I sought in vain for such a perforation, as that hinted at in the other account; but thro' the great magnifying power of this instrument, we were able to discover a gap or notch in the mountains to the westward, which abutted against the first streak or stream, and pursuing our object with great attention, we also perceiv'd a like gap in the direction of the lower streak: but tho' this streak was divided into two, we were not able at any rate to find out another notch, whereby

to

to account satisfactorily for the whole appearance; which I should have looked upon as solved, could such an one have been discerned in a right situation. But here I beg to refer myself to the judgment of this Society: only shall observe, the two gaps we saw were directly interpos'd between the sun and their respective streaks.

J. Short.

XXIV. *A Catalogue of the Fifty Plants from Chelsea-Garden, presented to the Royal Society, by the worshipful Company of Apothecaries for the Year 1750, pursuant to the Direction of Sir Hans Sloane, Baronet, Med. Reg. & Soc. Reg. nuper Præses, by John Wilmer, M. D. clariss. Societat. Pharmaceut. Lond. Soc. Hort. Chelf. Præfect. et Prælect. Botanic.*

- Read May 2. 1401. **A** Butilon periplocæ acutiori folio, fructu stellato. Hort. Elt. 1758.
- 1402 Allium saxatile acori radice flore purpureo Bocc. Mus.
- 1403 Androsace vulgaris latifolia annua Tourn. 123.
- 1404 Anemonospermus African. fol. & facie Taraxaci incanis Boerh.
- 1405 Aster Tripolii flore C. B.
- 1406 Aster cœruleus serotinus frutescens Tradescant.

- 1407 Balsamita fol. Agerati Vahl.  
Santolina spinosa fol. Agerati Boerh.
- 1408 Carduus albis maculis notatus exotico flore albo  
C. B. 381.
- 1409 Camara foliis subrotundis rugosis flore cœruleo  
Houft.
- 1410 Carduus Creticus Rapi folio Inst. R. H.
- 1411 Cirsium tuberosum Lactucæ capitulis spicatis  
Hort. Elt.
- 1412 Cnicus exiguus capite cancellato semine tomen-  
toso T. Inst.
- 1413 Cnicus orientalis Atractylis lutea dictus altif-  
simus T. Cor. 33.
- 1414 Cnicus orientalis Atractylidis folio flore Leuco-  
phæo T. Cor.
- 1415 Collinsonia Americana Urticæ foliis floribus ex  
albo purpur. Dillen.
- 1416 Corindum ampliore folio fructu majore T. 431.
- 1417 Corindum folio et fructu minori Tourn.
- 1418 Elichrysum African. fœtidissim. calyce argenteo  
Tourn.
- 1419 Eryngium montanum Amethyftinum capitulo  
majore pallescente T. 327.
- 1420 Ferula fol. glauco semine lato oblongo quibus-  
dam Thapsia ferulacea C. B.
- 1421 Hesperis Leucoid folio non serrato filiqua qua-  
drangula J. R. H. 223.
- 1422 Hieracium villosum Sonchus lanatus Dale-  
champio dictum R. H. 231.
- 1423 Hieracium Alpinum Scorzonæræ folio Inst. R.  
H. 1472.
- 1424 Horminum Napi folio Mor. Hort. Reg. Bleff.
- 1425 Jacea spinosa alato caule capite lanuginoso  
C. B. P.

- 1426 *Jacea angustifolia* minor *Virginiana* tuberosa  
radice Banister
- 1427 *Jacea* fol. candicantibus laciniatis caliculis non  
splendentibus
- 1428 *Lychnidea Mariana* elatior *Alfinesaquatic.* foliis  
floribus in longam spicam dense stipatis  
Pluk.
- 1429 *Lychnis orient.* annua supina *Antirrhini* fol.  
fl. min. purp.
- 1430 *Lychnis viscosa* flore muscoso *Ocimastri* facie  
C. B. P.
- 1431 *Marrubium album* candidissimum et villosum  
T. Cor.
- 1432 *Martynia* foliis ferratis Lin. Hort. Cliff.
- 1433 *Medica marina* major spinosa Park. Theat.
- 1434 *Medicago Vulnerariæ* facie *Hispanica* Inst. R.  
H. 412.
- 1435 *Mimosa humilis* frutescens et spinosa filiquis  
conglobatis
- 1436 *Moldavica Betonicæ* fol. floribus majoribus  
cœruleis pendulis Am.
- 1437 *Oryza* Lobel. Icon. 31. Offic. 336.
- 1438 *Plantago maxima* *Tartarica* Gerberi
- 1439 *Scolymus Chrysanthemus* annuus A. R. Par.  
III.
- 1440 *Scorpioides Bupleuri* fol. corniculis asperis ma-  
gis in se contortis et convolutis Mor. Ilist.
- 1441 *Sideritis Hispanica* frutescens seu lignosior I.  
R. H. 192.
- 1442 *Sonchus Lusitanicus Asplenii* folio
- 1443 *Stachys Lychnitis* Clusii
- 1444 *Tithymal. arboreus* altissimus fol. falicis cauli-  
bus rubentibus Boer.

- 1445 *Trifolium globosum repens* C. B. P. 329  
 1446 *Tordylium maximum* Inst. R. H. 320.  
 1447 *Tragofelinum maximum Austriacum foliis magis incis* Boer.  
 1448 *Valeriana Lusitanica latifolia annua laciniata* Tourn. 132.  
 1449 *Verbena tenuifolia* C. B.  
 1450 *Urtica racemifera maxima Sinarum foliis subtus argentea lanugine villosis.* Pluk. Almag. 212.

XXV. *Some Observations upon the Sex of Flowers by W. Watson, F. R. S. occasioned by a Letter upon the same Subject, by Mr. Mylius of Berlin.*

*Extract of Mr. Mylius's Letter to Mr. Watson, dated at Berlin, Feb. 20, 1750-51.*

Read May 2. 1751. “ **T**HE sex of plants is very well confirmed by an experiment, that has been made here on the *palma major foliis flabelliformibus*. There is a great tree of this kind in the garden of the royal academy. It has flower'd and bore fruit these thirty years; but the fruit never ripen'd; and when planted, it did not vegetate. The palm-tree, as you know, is a *planta dioecia*; that is, one of those, in which the male and female parts of generation are upon different plants. We having therefore no male plant, the  
 Y “ flowers

“ flowers of our female were never impregnated by  
 “ the *farina* of the male. There is a male plant of  
 “ this kind in a garden at Leipzig, twenty German  
 “ miles from Berlin. We procured from thence in  
 “ April 1749 a branch of male flowers, and sus-  
 “ pended it over our female ones; and our experi-  
 “ ment succeeded so well, that our palm-tree pro-  
 “ duced more than an hundred perfectly ripe fruit;  
 “ from which we have already eleven young palm-  
 “ trees. This experiment was repeated last year,  
 “ and our palm-tree bore above two thousand ripe  
 “ fruit. As I do not remember a like experiment,  
 “ I thought convenient to mention it to you; and,  
 “ if you think proper, be pleased to communicate  
 “ it to the Royal Society.”

In pursuance of my correspondent's desire, I take the liberty of laying this account before you, which I think very curious; not on account of its novelty, or of its confirming the sex of plants, which is now sufficiently established; but on account of the male and female palm-tree's flourishing so completely, even under all possible advantages, in such high latitudes as those of Leipzig and Berlin.

The impregnation of the female palm-tree by the male has been known in the most ancient times. Herodotus \*, whom Cicero calls the father of history, when

\* Herodot. ΚΑ. ι. ε.

Γεν τε ἄλλα καὶ φοινίκων τὰς ὕψους Ἑλληνες καλῶσι, τῶν τὸν καρτὸν  
 ἔχοντων τῶν ἑλλὰσι ἰσχυροῦσι τὸν συνήθειον, ἵνα πεποινησὶ σφί φῶν τὴν  
 ἡλικίαν ἐσθλότερον, καὶ μὴ ἀπορρέῃ ὁ καρτὶς οὗτοι ποσειδῶνος φῶνα γὰρ δὲ  
 ἡ φύσις ἐστὶν καὶ τῶν ἄλλων φασκεῖται, καὶ οὕτως ὁ δὲ ἑὸν ἔκγονον εἶναι.

when speaking of the palm-tree, says, “ that the  
 “ Greeks call some of these trees male, the fruit of  
 “ which they bind to the other kind, which bears  
 “ dates; that the small flies, wherewith the male  
 “ abounds, may assist in ripening the fruit; for, says  
 “ this author, the male palm-tree produces in its  
 “ fruit small flies, just as the fig-tree does.” The very  
 remote age, in which Herodotus wrote, sufficiently  
 apologizes for his believing, that what was really  
 brought about by the *farina fecundans* of the male  
 flower, was to be attributed to the insects frequently  
 found therein, and which perhaps very often do carry  
 this *farina* from the male to the female. They had  
 seen the effects of caprification in fig-trees by these  
 insects, and were misled by the analogy. I have here  
 translated them small flies, but they had a particular  
 appellation given them by Herodotus, Aristotle \*,  
 and Theophrastus, who call them *ψύλλαι*. Pliny, in  
 his history, when treating of caprification, which is  
 almost a translation from Theophrastus, calls them  
*culices*, Linnæus *ichneumones*, and Tournefort *mou-*  
*cherons*.

Theophrastus §, the most early writer of plants,  
 except Aristotle, that has been handed down to us,  
 in his account of the palm-tree gives us the very  
 process mentioned by our correspondent. “ They  
 “ bring together (says this author) the males and  
 “ the females, which causes the fruit to continue,  
 “ and ripen upon the trees. Some, from the simili-

Y 2

“ tude

\* Aristoteles περὶ ζώων. Οἱ δὲ ἐρῶναι --- ἔχουσι τὸς καλεμένους  
*ψύλλας*.

§ Theoph. περὶ φυτῶν. Κεφ. θ.



“ tude of this to what happens in fig-trees, call it  
 “ caprification ; and it is performed in the following  
 “ manner : While the male plant is in flower, they  
 “ cut off a branch of these flowers, and scatter the  
 “ dust and down therein upon the flowers of the  
 “ female plant. By these means,” he goes on, “ the  
 “ female does not cast her fruit, but preserves them  
 “ to maturity.” Pliny\* also mentions the like pro-  
 cess.

Among more modern authors, Prosper Alpinus † gives us at large the manner of the impregnation of the female palm-tree by the male, for the purposes before-mentioned. We have also copious accounts of the same process by Tournefort §, Kämpfer ||, and Ludwig \*\*. As Kämpfer was an eye-witness, his account of this matter is most to be depended upon. He says, “ Plena res dignissimaque admirationis est  
 “ modus palmas fœmininas fœcundandi. Habet id  
 “ tot popularium, Perfidis, Arabiæ, Ægypti, nutrix  
 “ inter plantas singulare, ut animalium exemplo,  
 “ mari statuto tempore miscenda, atque singuli ejus  
 “ uteri, quasi conjugali coitu, impregnandi sint ; se-  
 “ cus omnia sua, quæ in lucem prodiderat, fructuum  
 “ rudimenta, indeclinabili abortu dimissura. Palmi-  
 “ colis itaque incumbit, ut impregnandis arboribus  
 “ quotannis impendant operam, siquidem in se re-  
 “ dundare annonam cupiunt. Modus procedendi  
 “ hic

\* Plinii Hist. Nat. lib. xiii. cap. iv.

† Alpin. de plant. Ægypt. p. 16.

§ Isagog. instit. rei herbar. p. 69.

|| Amœn. exot. p. 706.

\*\* Dissert. de sexu plant. p. 29.

“ hic est : spaltæ masculæ incluso tumentes flore, et  
 “ ad thalami consortium maturo, sub finem Februarii  
 “ ex arboris fastigio extrahuntur ; quibus in longum  
 “ dissectis eximuntur spadices, flosculis nondum of-  
 “ citantibus, sed in unam massam compactis con-  
 “ ferti. Hos protinus in furculos live bacillos, spa-  
 “ dicibus fæmininis inferendos diveliunt. Bacillos  
 “ alii amant recentes, atque illico insinuare spadici-  
 “ bus, si qui jam lucem nati sunt ; lii eos prius  
 “ exsiccant, et in Martium usque mensem custodiant,  
 “ quo hiantibus uteris ad unum omnibus infitionem  
 “ uno actu et opera instituant.”

As I am now upon the sex of plants, I cannot but observe, that although the ancients distinguished rightly, in determining the true sexes of the palm-tree, it is the only plant, in which they have not erred. Though they called plants of the same *genus*, or of others very nearly related thereto, male and female, it was upon an imaginary, a false principle ; and that usually taken from their size, the difference of their leaves, or the figure of their fruit ; and what therefore they have denominated male and female, must not with the modern exactness be rigorously considered as such. Thus Aristotle \*, after having taken notice that there was the distinction of male and female observable in plants, says, “ that the male ♂ plant  
 “ is more rough and strong, the female more weak  
 “ and fruitful.” And Theophrastus ||, when speak-  
 ing

\* De plant. lib. 1. cap. 2.

ἐν τῇ δὲ ἐν τοῖς φυτοῖς, οἷς ἔχει τὰ φύλα γένος ἄρρεν καὶ θῆλυ.

§ Aristot. ibid.

|| Plantar. histor. lib. iii. cap. 10.

ing of the male and female pine-tree, says, “ that  
 “ the Macedonians have trees nearly related to pines,  
 “ of which the male is of shorter growth, and has  
 “ larder leaves; that the female is taller, and has  
 “ its leaves softer, and more fleshy.” He says, upon  
 his own authority, “ that the wood of the male pine  
 “ is hard, that of the female more soft.” Pliny \*  
 also in his history gives a like reason for his distin-  
 guishing the sex of the pine: he says farther §, in  
 another part of the valuable monument he has left us,  
 “ that the most expert naturalists assert, that every  
 “ tree, and every herb, which the earth produces,  
 “ hath both sexes:” but this is to be understood in  
 the manner I just now mentioned; and so likewise  
 is the distinction among the more modern botanists  
 in their denominations of several plants, such as *Veronica*,  
*Eupatorium*, *Anagallis*, *Tilia*, *Pæonia*, *Bal-*  
*samita*, *Filix*, *Quercus*, *Orchis*, *Laureola*, *Abro-*  
*tanum*, *Cornus*, *Polygonum*, *Equisetum*, *Mandragora*,  
 and others, which are termed imaginarily male and  
 female; as the discovery of the real sex of plants  
 was reserved for the accuracy of the present age.

Besides the before-mention'd erroneous principle,  
 from which the antients, as well as some more mo-  
 dern authors, determined the sex of plants, there is  
 yet another, which I think right to mention in this  
 place; and that is, a denomination of plants from  
 their sex, which is absolutely false: and in order to  
 elucidate this position, and to shew at the same time  
 wherein

\* Lib. xvi. cap. 10.

§ Lib. xiii. cap. 4.

whercin the sex of plants does really consist, I must beg leave to premise, that it is in the flowers of vegetables only, that the parts subservient to generation are produced. Simple flowers (I use this term in opposition to the compound flowers of the botanists) are either male, female, or hermaphrodite. By male flowers, I would be understood to mean those, which are possessed only of those organs of generation, analogous to the male parts of animals; and these are, what former botanists have denominated *stamina* and *apices*, but are nam'd more properly by Linnæus since, *filamentum* and *anthera*. The female flower is only endowed with parts like those, which perform the office of generation in females; and these are the *pistillum* and its appertenances, which by Linnæus, with his accustomed accuracy, are divided into three parts; viz. the *germen*, *stylus*, and *stigma*. The hermaphrodite flower, which constitutes the great bulk of the vegetable creation, is possessed of all these parts in itself, and is itself thereby capable of propagating its species without any foreign assistance; which, by many incontestable experiments it has been found neither the male nor female flower simply is able to do.

Much the greater number of plants, as I have just hinted, have hermaphrodite flowers; but there are some, which have both the male and female flowers growing from the same root. Such are *Mays* or Indian corn, nettles, box, elm, birch, oak, walnut, beech, hazel, hornbeam, the plane-tree, pine, fir, cypress, cedar, the larch-tree, melons, cucumers, gourds, and several others. In many of these, though the male and female flowers are at considerable distances,

distances, the *farina fœcundans*, which Providence, on account of its being liable to be spoiled by rain, or dissipated by winds, has provided in great abundance, is conveyed to the female by means of the atmosphere. It is this class of vegetables, and the following, the quantity of the produce of which is much more precarious than those plants, which have hermaphrodite flowers; as the impregnation of these last may be performed within their own calyx; whereas the former must necessarily commit their *farina* to the circumambient air. It is for this reason, that if during the time of the flowering of these plants, the weather is either very wet or stormy, their produce of fruit will be very inconsiderable, from the spoiling or hasty dissipation of the male *farina*. Thus independent of frosts, the fruit of the nut and filbert-tree will be most numerous in those years, in which the months of January and February are the least stormy and wet; as at that time their flowers are produced. For the same reasons, a stormy or wet May destroys the chesnuts; and the same weather in July prodigiously lessens the crop of *Mays* or Indian corn, as its spikes of male flowers stand lofty, and at a considerable distance from the female. In like manner a judgment may be formed of the rest of these.

Some of the more skilful modern gardeners put in practice, with regard to melons and cucumers, the very method mention'd by Theophrastus 2000 years ago, in regard to the palm-tree. As these plants, early in the season, are in this climate confined to frames and glasses, the air, in which they grow, is more stagnant than the open air, whereby the distribution  
of

of the *farina fœcundans*, so necessary towards the production of the fruit for the propagation of the species, is much hindered; to obviate which, they collect the male flowers when fully blown, and presenting them to the female ones, by a stroke of the finger they scatter the *farina fœcundans* therein, and this prevents the falling of the fruit immaturity.

Besides the vegetables before-mentioned, which bear both male and female flowers upon the same root, there are others, which produce those necessary organs upon different roots. In the number of these are the palm-tree (the more particular subject of this paper) hops, the willow-tree, mistletoe, spinach, hemp, poplar, French and dog's mercury, the yew-tree, juniper, and several others. Among these the *Valisneria* of Linnæus, as to the manner, in which its male flower impregnates the female, is one of the most singular prodigies in nature. The manner of this operation is figured by Micheli, in his *Nova plantarum genera*, and described by Linnæus, in the *Hortus Cliffortianus*. As that elaborate and expensive work is in very few hands, in such only as owe it to the munificence of Mr. Clifford of Amsterdam, of which number I with pleasure acknowledge myself one, I will here lay before you a short account thereof:

The *Valisneria* grows in rivulets, ditches, and ponds, in many parts of Europe. The male plant, which is continually covered with water, has a short stalk, upon the top of which its flowers are produced. As this top never reaches the surface of the water, the flowers are thrown off from it, and come unopened to the surface of the water; where, as

soon as they arrive, by the action of the air, they expand themselves, and swim round the female flowers, which are blown at the same time. These last have a long spiral foot-stalk, by which they attain the surface of the water, and remaining there in flower a few days, are impregnated by the male flowers detached from the stalk at the bottom. This operation seems to be thus directed, as the *farina fœcundans* could not exert its effects in so dense a medium as water ; and we find, that even the hermaphrodite flowers of water-plants, such as those of *potamogeton*, *ranunculus aquaticus*, *bottonia*, and *nymphæa*, these, I say, never expand themselves, until they reach the surface of the water.

But to return : it was not possible for me, without premising these things, to make evident what I just now mention'd, in relation to the falsely denominating the sexes of plants ; as it is to this last class that the wrong application has been made by botanical writers. This error seems to have been first introduced so early as by Dioscorides, and has been continued through a great variety of writers even to our own time. It is most certain, that those plants, which produce the seed, ought to be considered as females ; but it happens that in the French and dog's mercury, the seeds are produced in the female plants by pairs ; and these are contained in a capsule, which was thought to resemble the *scrotum* of animals ; and from this testiculated appearance they called these plants males, and the others females.

Thus, for example, Dioscorides\*, when treating of *mercurialis*, or what we here call French mercury, says, that “ the seed of the female is produced in bunches, and is copious; that of the male grows near the leaves; that it is small and round, and is disposed in pairs like testicles.” Dodonæus, Lobel, Dalechamp, John and Caspar Bauhin, Morrifon, Tournefort, and Boerhaave, in their several works, have in this followed Dioscorides, and have denominated the seed-bearing plant of this kind, the male; and the other, the female. Fuchsius and John Bauhin likewise call the *cynocrambe* or dog’s mercury, which bears fruit, the male; and the spiked one with male flowers only, the female. This mistake is observable in hemp §, hops, and spinach.

We observe, that the operations of nature are carried on most usually by certain general laws, from which however she sometimes deviates. Thus almost all plants have either hermaphrodite flowers, or male and female flowers growing from the same root, or male and female flowers from different roots: but there are a few of another class, which from the same root furnish either male and hermaphrodite flowers, or female and hermaphrodite flowers. Of this kind are the mulberry-tree, the *musa* or plantain-tree, white hellebore, pellitory, arrach, the ash-tree, and a few others. But of this class the *empetrum*

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or

\* Dioscorid. lib. iv. cap. 9. edit. Saracen.

Λινόζασις οἱ δὲ παρθένιον, οἱ δὲ Ἑρμὺς ἐσάνιον καλεῖται . . . . τὸν δὲ παρθένον ἢ μὲν θάλασσα βότρυσι δ’ ἢ καὶ πολὺν· ὁ δὲ ἄρρην πρὸς τοῖς πετάλοις μικρὸν, τρογγύλοι, ὥσπερ ὀρχιδία καὶ δύο περσικόμενα . . .

§ Matthioli. in Dioscorid. p. 663. semen tantum in mari gignitur;



or berry-bearing heath is the most extraordinary ; as of this are found some plants with male flowers only. others with both male and female flowers separately, and still others with hermaphrodite flowers.

What Pere Labat mentions in his *Voyage à l'Afrique occidentale* should likewise be taken notice of here. This author, after having laid down the different methods of impregnating the female palm-tree by the male, says, that this process is not absolutely necessary for the production of dates ; for being at Martinico, he there saw growing by an old convent near the place, where they anchored, a palm-tree bearing dates, although the only one of its kind which was thereabouts. Whether it was male or female, he did not pretend to determine, but was certain, that there then was none, nor had been one, within two leagues of the place where it grew. He doubts indeed, whether or no this tree bearing fruit did not proceed from the *farina fecundans* of the male cocoa tree, which is a species of palm, and which grew in abundance near the tree that bore date : but he observes, that the stones of these dates did not vegetate, and that those, who were desirous of propagating date-trees, were obliged to plant the Barbary dates ; as he believed the others had not the germ proper to produce the tree. From this account it is very obvious, that the palm-tree here mentioned was a female, in which though the fruit ripened, it was in such a state of imperfection, as not to be able to propagate its species. In this manner we have eggs furnished us by hens, even without a cock ; but these eggs produce no chickens. What this father says of the female palm-tree's bearing fruit without

without the assistance of the male, our very ingenious and worthy brother Mr. Miller assures me, has been fully confirmed to him by several persons : and John Bauhin\*, an author of great credit, describes and figures the whole fructification of a palm-tree, which himself saw growing at Montpellier, and which not only produced branches of male flowers, but also female one bearing dates. Mr Ray many years after tells us in his history of plants §, that he himself at Montpellier saw this very remarkable tree mentioned by John Bauhin.

This variety in the fructification of the palm tree, singular as it may seem, has been likewise observed in some few others. The learned Jungius, in his *Doxologia* ||. mentioning that class of trees, which are male and female in different parts of the same tree, says, “ that trees of this kind, when they “ have for many years produced flowers without “ fruit afterwards produce fruit without flowers. “ This, he thinks, should be further inquired into.” This, since Jungius’s time, has been done, and it has been found that sometimes some of the trees of this class are wholly male, while young ; but as they advance in age, they have flowers of both sexes, and afterwards become intirely female. This fact Mr. Miller has frequently himself observed in the mu’berry-tree ; and the Chevalier Rathgeb, at present the emperor’s minister at Venice,

\* Hist. plant. tom. i. p. 351.

§ Rati hist. plant. tom. ii. p. 1354.

|| Cap. iv. p. 145. *Ubi aliquot annos flores tulerunt sine fructu, deinde fructus ferre sine flore, quod amplius observandum.*

nice, a gentleman excellently well versed in whatever relates to vegetation, has observed, that a large *lentiscus*, or mastich-tree, near his garden, had for thirty years produced only male flowers, but that for three years past it had produced plenty of fruit.

The foundation of the discovery of the real sex of plants, which is of no less importance in natural history, than that of the circulation of the blood in the animal œconomy, was laid by the members of this learned Society; although much of the honour due to them is attributed by foreigners to the late ingenious Monsieur Vaillant of Paris: and this may have arisen from our language not being generally understood upon the continent. Sir Thomas Millington \*, sometime Sedleian lecturer of natural philosophy at Oxford, as we see by our worthy member Dr. Grew's anatomy of plants §, seems first to have assigned a more noble purpose to the *stamina* and *apices* of flowers, than that which had been attributed thereto by preceding writers, and by Monsieur Tournefort afterwards; viz. that of secreting some excrementitious juices, which were supposed hurtful to the embryo's of the fruit. Sir Thomas conjectured, and rightly, " that the *stamina* " and *apices* served as the male for the generation " of seed." This hint, which was afterwards adopted by our learned brother Mr. Ray, in the preface to

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\* Dr. Grew calls Sir 'Thomas Millington Savilian professor, which is a mistake. See *Wood's Fasti. Oxon.* vol. ii. col. 126. 2d edit.

§ Page 171.

to his *Sylloge stirpium exterarum*, Dr. Grew carried farther, as we find by his works; and it was followed by || Rodolphus Jacobus Camerarius, professor at Tubingen: but our very industrious and sagacious member Mr. Morland \* pursued long after this inquiry still much higher, as we see by his excellent memoir published in the *Philosophical Transactions*, to which I must beg leave to refer you. After these, Messieurs Vaillant and Geoffroy illustrated and strengthened these discoveries by very curious and well adapted experiments; so that at present nothing seems wanting for the confirmation of the truth of this doctrine.

So much for the discovery of the sex of plants in general, upon which professor Linnæus of Upsal has founded his system of botany, at present so much and so well received. Whoever therefore would consider minutely the structure of flowers and the almost infinite variety of the number and disposition of their parts, may consult Linnæus's *Philosophia botanica* lately published, where this subject is treated in a very copious and instructive manner.

|| Vide epistol. de sexu plant. Tubing 1694.

\* Philosoph. Transf. numb. 287.

XXVI. *Two Letters of Mr. John Harrifon of Cambridge in New England, to Mr. Peter Collinfon, F R. S. concerning a fmall Species of Wafps.*

S I R,

Cambridge, 29 July, 1748.

Read May 9.

1751.

**A**BOUT the 28 of May laft, I difcover'd hanging to the roof on the infide of my green houfe (which is of wood) fomething about the fize of a child's farthing ball, in fhape like a Provence rofe full-grown, before it opens, that is, a round bottom, ending in a blunt point; at which point is a round hole, large enough for infects (fomething lefs than a wafp) to go in and out at. I foon perceiv'd, that it was the work of infects, a fmall fpecies of wafps. They have fix legs, black next to their body, then yellow, ending in cinnamon-colour. Some have 6 and 7 rings, of a bright yellow colour, round the tail part of their body, with fmall hollows or indents on the upper parts. The divifions between the rings are of a bright jet colour; the face is yellow; on the head are two horns.

Thefe little infects are very induftrious in making their neft. The top of it is faften'd or glewed to the cieling, and is formed of many round coverings, one within another, yet not touching each other, by the 8 part of an inch. Probably this fpace is left to make their cells, in which they lay their eggs. Thefe coverings have been repeated until there are now thirteen finifhed, ranging equally one over another.

It

It is most curious to see their manner of working. As this performance is most externally, I have an opportunity of seeing every minute circumstance of this operation, which is carried on with as much pains and application, as (but I think I may say with more skill and contrivance than) the honey-bees, who are beholden to a hive or hollow tree, &c. to fabricate their combs in; whereas these little animals are the sole builders of the outward walls, as well as the interior parts of their dwellings. They range about for the materials, but with all my endeavours I could never observe, from whence they were collected; only this I know, that they bring a little lump of dark-colour'd paste between their fore-legs, about the size of a radish-feed. This they carry first to the inside of the covering, which they are about to finish, and stay near half a minute, I suppose to work some of it on that side: then they return with the greatest part, to enlarge it on the outside, which they execute in a most dextrous manner (as I have many times seen) by taking the paste from between their legs with their mouths (which open cross ways to their body) and fixing it on the edge of the covering, working backwards, for about an inch at a time in length, and then spread and smooth it with their horns. This is all performed in about two minutes, and they are seldom more than five days in finishing a whole cover. By the nicest observation I could make, their number is between 20 and 30. They seem no-ways hurtful; and are so intent on their business, that if 3 or 4 people at a time are looking within so many inches of their nest, they neither attack them, nor forbear to carry on the

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public work, which is now 5 inches diameter, and about 4 deep. In my next you shall hear further how this little colony goes on. I am, Sir,

Your most humble servant,

John Harrison.

S I R,

Cambridge, Dec. 22 1748.

**W**HAT I have further to add to my former observations on the pretty insects, that were building their nest in my green-house, is, that they continued their work, in the same manner as before-mention'd, untill they had finished 15 coverings one over another, and began three more, which they never completed, but one is more finished than the others.

About the 16 of August there was a cessation of their usual industry. I could only observe one or two in a day at work, which continued to the 26, when they quite gave over adding any more to their nest. Since that, I could only see one or two going in and out once or twice a day, for about a fortnight after. In that time I observed two of these insects come out of their nest, of an extraordinary size, at least one third larger than those, that built the nest. These seem to me, and undoubtedly are, the parents or queens appointed by the all-wise Creator for continuing their species, as their sluggishness has a near analogy to the queen-bees, that are sometimes seen to come to the mouth of the hive, without any other seeming business than to take the air, and shew themselves,

themselves, and then return into the hive again. About the 6 or 7 of September, I saw the last; none have since been seen.

As these insects are new to me, and to all who have seen them, I cannot say any thing certain of their future progress; but, if I may compare them to, as they most resemble the hornets, in their making and hanging up of their nest, the queens will only survive, and each in the next spring be the founder of a new colony. The common wasps are under the same regulation. The males all die at the approach of winter, and leave but very few females to survive them. This is wonderfully contrived to prevent the increase of such noxious animals; whereas the bees, so beneficial to mankind, survive the winter, unless robbed of their honey, which is their support during that season.

I have had at least 500 learned gentlemen of this university to see these insects, and their operations. Is it not very remarkable? Not any one of them had ever seen the like, or could give any similar account of any thing of this nature.

I have waited with impatience the coming of the spring; but, to my great disappointment, none of my pretty little inmates returned to their nest; which makes me conclude, that it is their annual work. This determined me to take it down carefully; and as I promised to send it you, I desire your acceptance of it, and of one of the insects. I hope it will prove an agreeable entertainment to you and your curious friends. I am, Sir,

Your most obliged humble servant,

John Harrison.



XXVII. *A Letter from Dr. T. Coc, Physician at Chelmsford in Essex, to Dr. Cromwell Mortimer, Secr. R. S. concerning Mr. Bright, the fat Man at Malden in Essex.*

S I R,

Chelmsford, April 16, 1751.

Read May 9.  
1751.

**I** NOW send you a plain but true and authentic account of an extraordinary man, whom you yourself have seen, and whom I have known ever since he was a boy, viz. Mr. Edward Bright, grocer, late of Malden in Essex, who died there the 10 of November last in the 30 year of his age. He was a man so extremely fat, and of such an uncommon bulk and weight, that I believe there are very few, if any, such instances to be found in any country, or upon record in any books; at least I have never heard or read any genuine account of a man, who was equal, or even came near to him in weight. I know, that Dr. Allen, in his *Synopsis universæ medicinæ practicæ*, quotes Sennertus for a woman of 450 pounds, and for a man, who weighed 600 pounds; and Chambers, in his Dictionary, mentions the same precisely in the same way; which therefore I suppose he took from Allen. But the numbers are falsely printed in Allen; for, as they stand in Sennertus *cap. de corpulentia nimia*, the weight of the woman is 480 pounds, and that of the man several pounds more than 400.

If the following story of Mr. Bright should exceed the faith of any in this present age, there are a great many

many witnesses, who can attest it: and if posterity shall find themselves at a loss to believe it, upon this bare relation, they may have further evidence, if they will be at the pains to consult a public record of the corporation, made by the order of the present magistrates; and also the register of the parish of All Saints in Malden, where he was buried; in both which they will find the main facts properly vouched.

Mr. Bright was descended from families greatly inclined to corpulency, both on his father's and his mother's side. Many of his ancestors and relations have been remarkably fat, though very far inferior to him in bulk. He was always fat from a child, and yet very strong and active, and used a great deal of exercise, both when a boy, and after he became a man, which he continued to do till within the last two or three years of his life, when he became too unwieldy. He could walk about very well, and nimbly too, having great strength of muscles; and could not only ride on horseback, but would sometimes gallop after he was grown to between 30 and 40 stones weight. He used to go to London about his business, till the journey of 40 miles, and going about there, became too great a fatigue to him; and he left it off for some years before he died. But he was grown to such a size before he left it off, that he was the gazing-stock and admiration of all people, as he walked along the streets. In the last year or two he could walk but a little way, being soon tired, and out of breath, and travelled abroad but little, and that in a chaise. He was so large and fat a boy, that at the age of 12 years and a half he weighed 10 stones and 4 pounds horse-  
man's

man's weight, *i. e.* 144 pounds\*. And he increased in bulk, as he grew up, so that in seven years more, that is before he was twenty, he weighed 24 stones, or 336 pounds. He went on increasing, and probably in pretty near the same proportion. For the last time he was weighed, which was about thirteen months before he died, his weight was 42 stones and 12 pounds, with only his waistcoat, shirt, breeches, and stockings on; and these cloaths being afterwards weighed, were found to be 16 pounds; so that his neat weight at that time was 41 stones and 10 pounds, or 584 pounds. What his exact weight was at the time of his death, cannot be told: but, as he was manifestly grown bigger since the last weighing, which he himself, and every body about him, were sensible of, if we take the same proportion, by which he had increased for many years upon an average, *viz.* of about 2 stones a year, and only allow 4 pounds addition for last year, on account of his moving about but very little, while he continued to eat and drink as before (which allowance is perhaps less might be granted) this will bring him to 44 stones or 616 pounds neat weight. And that I find by the judgment of the most reasonable people, who knew him well, and saw him often, is reckoned a very fair and modest computation, and the lowest, that can be made.

As to his measure, he was 5 feet 9 inches and a half high. His body round the chest just under the arms  
measured

\* There is at this time at Malden a boy not 14 years old (no relation to Mr. Bright) who weighs as much. *Tulpius Obs. medic. lib. 3. cap. 55.* tells of a boy of vast bulk and strength, who at 5 years of age, weighed 150 pounds; but does not say what became of him afterwards.

measured 5 feet 6 inches, and round the belly 6 feet 11 inches. His arm in the middle of it was 2 feet 2 inches about, and his leg 2 feet 8 inches.

He had always a good appetite, and, when a youth, used to eat somewhat remarkably ; but of late years, though he continued to eat heartily, and with a good relish, yet he did not eat more in quantity than many other men, who, we say, have good stomachs.

As to drink, though he did not take any liquor to an intoxicating degree, yet perhaps upon the whole he drank more, than might have been adviseable to a man of his very corpulent disposition. When he was a very young man, he was fond of ale and old strong beer ; but for some years past his chief liquor was small beer, of which he commonly drank about a gallon in a day. In other liquors he was extremely moderate, when by himself, sometimes drinking half a pint of wine after dinner, or a little punch, and seldom exceeding his quantity ; but when he was in company, he did not confine himself to so small an allowance.

He enjoyed for the most part all his life as good health as any man, except that in the last 3 years, he was two or three times seized with an inflammation in his leg, attended with a little fever ; and every time with such a tendency to mortification, as to make it necessary to scarify the part. But by the help of scarification and fomentations, bleeding largely once or twice in the arm, and purging, he was always soon relieved. I say bleeding largely, for it was always the custom with him, to have not less than two pounds of blood taken away at a time. And he

was

was no more sensible of the loss of such a quantity, than another man is of twelve or fourteen ounces.

He married when he was between twenty-two and twenty-three years old, and lived a little more than seven years in that state: in which time he had five children born, and left his wife with child of the sixth, near her time.

There was an amiable mind in this extraordinary overgrown body. He was of a chearful temper, and a good-natured man, a kind husband, a tender father, a good master, a friendly neighbour, and a very fair honest man. So that he was beloved and respected by all, who knew him, and would have been as much lamented by his acquaintance, as any man in any station of life ever was, had it not been, that they looked upon him for several years as a man, who could not live long; and out of regard and compassion to him, considered his life as a burthen, and death as a happy release to him, and so much the more, as he thought so himself, and wished to be released.

His last illness, which continued about fourteen days, was a miliary fever, as I am well informed by the apothecary, who attended him. It began with pretty strong inflammatory symptoms, a very troublesome cough, great difficulty of breathing, &c. and the eruption was extremely violent. For some days he was thought to be relieved in the other symptoms by the eruption: but it seems to be no wonder at all, that his constitution was not able to struggle through such a disease, which proves so fatal to many, who appear to be much more fit to grapple with it.

His

His body began to putrify, very soon after he was dead ; so that notwithstanding the weather was cool, it became very offensive the next day, before they could get a coffin made. As the corps was of a surprising bulk, the coffin must be so too. It was 3 feet 6 inches broad at the shoulders, 2 feet 3 inches and a half at the head, 22 inches at the feet, and 3 feet 1 inch and a half deep.

Great numbers of people came to see the coffin, while it was making ; and at the funeral there was a vast concourse, not only of the town, but from the country for several miles round about, out of curiosity to see, how such a corps could be got to the ground. It was drawn to the church on a low-wheel'd carriage by ten or twelve men, and was let down into the grave by an engine fixed up in the church for that purpose. I am,

S I R,

Your most humble servant,

T. Coe.

XXVIII. *The Effects of the Hyoscyamus albus, or white Henbane; in a Letter to Dr. John Pringle, F. R. S. from Dr. John Stedman, late Surgeon-Major to the Regiment of the Royal Grey Dragoons.*

S. I R,

Edinburgh, Octob. 2, 1750.

Read May 16.  
1751.

**I**N the month of August 1748, whilst the Greys were cantoon'd in the village of Vucht near Boissleduc in Dutch Brabant, five men and two women of that regiment having eaten of the leaves of the *hyoscyamus albus*, shred and boiled in broth, were soon after seized with a giddiness and stupor, as if drunk. I saw them about three hours after having eat of it; and then three of the men were become quite insensible, did not know their comrades, talk'd incoherently, and were in as high a delirium, as people in the rage of a fever. All of them had low irregular pulses, flaver'd, and frequently chang'd colour: their eyes look'd fiery, and they catch'd at whatever lay next them, calling out, that it was going to fall. They complain'd of their legs being powerless. I mix'd what ippecacuana I had with me in warm water, and made them drink it; and afterwards threw in as much warm water and oil, as I could prevail with them to swallow. Those, who were not insensible, vomited freely, and were relieved by it. Two of the three affected with delirium, tho' they drank great quantities, did not vomit, but had profuse sweats, and pass'd plenty of urine,

urine, by which they were likewise somewhat relieved. The third of these was obstinate, nor could he prevail'd upon to do any thing. The symptoms with him continued longer, and were more violent. He was so restless, that, notwithstanding he could not walk, two of his comrades were not able to keep him in a chair. Next morning they had no other complaint than people commonly have after great drinking; but afterwards (tho' the danger seem'd over) some of them complain'd of feebleness and a weight at their stomachs; others, of gripe, stitches, headach; and all of them were vertiginous at times. These complaints continued above a month after the accident. One of the women had her hands stiff and swell'd; whether from the action of the vomit, or the force of the poison, I know not. The man, who pull'd these leaves in mistake for another plant, said, that from the nearest conjecture he could make, there might be from fifteen to twenty leaves, boil'd in about ten quarts of water. They did not eat one half of that quantity, and the poison began to discover itself with some of them in half an hour. This seem'd to be the *hyoscyamus major albus* of Caspar Bauhinus. It is easily known by its large dusky bell-flower; but if not in the flower, the remarkable noisome smell of the leaf, somewhat narcotic, if once known, will ever after discover it.

Some time before this accident, we had a proof of the effect of the yew-tree upon some of our horses: they were put into an orchard, where they cropp'd the branches of these trees, and about four hours after, without any previous symptom of disorder, dropp'd down, and after a struggle of a minute or



two died. This was probably about the time, that the juice enter'd the blood.

*Remarks by Mr. William Watſon, F. R. S.*

Upon reading the above paper, Mr. Watſon obſerv'd, that the effects therein mentioned could not ariſe from the *hyoſcyamus albus*, or white henbane, as Dr. Stedman imagines; that plant, from the concurrent teſtimony of the beſt botanical writers, not being found ſo far north as Brabant: but the miſchief was done by the *hyoſcyamus niger*, or black henbane, which grows plentifully there, as well as almoſt all over Europe in uncultivated places, and by the ſides of roads. The white on the contrary is ſown in gardens, and not found ſpontaneous in higher latitudes than the ſouthern parts of France.

Dr. Stedman's deſcription demonſtrates likewiſe the above plant to be the *hyoſcyamus niger*, as he ſays, that "it is known by its duſkiſh bell-flower." The flower of black henbane is of that hue, being of a yellow colour interſperſed with veins of purple; whereas the flower of the white henbane is of a pale-yellow colour.

This error ariſes from the improper denomination impoſed upon many plants by the ancients, and which has been preſerved even ſince the revival of letters; which, to one not very well acquainted with botany, is liable to miſlead. Thus, in the caſe before us, the leaves of the black henbane are very little leſs white than thoſe of the white; but this denomination took its riſe from the different colour of their ſeeds. In ſuch caſes therefore, without  
being

being well acquainted with the specific difference of each plant, before it ripens its seed, it is not a little difficult to distinguish them one from the other. This specific difference will be best furnished by the leaves. Thus in the henbane, the leaves of the white are placed upon long footstalks; those of the black have none, but the lower extremity of the leaf surrounds the stalk.

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**XXIX.** *The best Proportions for Steam-Engine Cylinders, of a given Content, consider'd; by Francis Blake, Esq; F. R. S.*

Read May 23 1751. **T**HE fire-engine, or (to term it more properly) the steam-engine, for draining of mines, is a master-piece of machinery, a very capital contrivance in the works of art, and meriting our attention for farther improvements. This is universally allowed, as well upon account of the theory it is founded on, as its usefulness in practice. And is it arriv'd then at the last degree of perfection, that we appear at a stand? I think not. The prodigious vessel of water to be kept always boiling, when only an inconsiderable part of it is employ'd in the work, favours too little of the frugality of nature, which we ought ever to imitate. But waving that now, what I would inquire into here, and endeavour to regulate, is, the cylinder's proportion of the altitude and base; which hath not, as I know of, been hitherto noticed.

It is evident, in the first place, from a fundamental law of mechanics, that, the content of the cylinder remaining the same, the quantity of water discharged at each lift will in all cases be equal, by only changing the distance of the center of the piston from the fulcrum of the balance. You will agree likewise (for I suppose the principles and working part to want no description) that the excess of the pillar of atmosphere above that of the water is a weight on the piston, driving it to a depth of five feet, or thereabout, by the present construction, with the cavity of the cylinder; acceleratedly till friction and an impediment from the steam, which remains in the cylinder even after the jet d'eau, and is increased in elasticity, whilst its bounds are diminished, shall equal the accelerative force; and that then again the piston is retarded the rest of the way. It may be convenient to remark too, that if the rarefaction be so complete, that the descent would be greater than the construction admits of, the retardation is augmented by a *brachium* of the balance pressing upon springs. But to say nothing of friction here, we can, notwithstanding this diminution of force by the remainder of steam within the cavity of the cylinder, demonstrate the ratio of the velocities, and the times of descent of the pistons, in cylinders of unequal altitudes, to be exactly the same, as if the resistance was nothing; whence we shall without difficulty arrive at some conclusion in this matter.

*MN* is the working-part of a steam-engine cylinder, of the usual height, equal in diameter to a shorter one *mn*; and the rarefaction in both of them being supposed the same,  $AQ=aq$ ,  $RQ=rq$ ,  
and

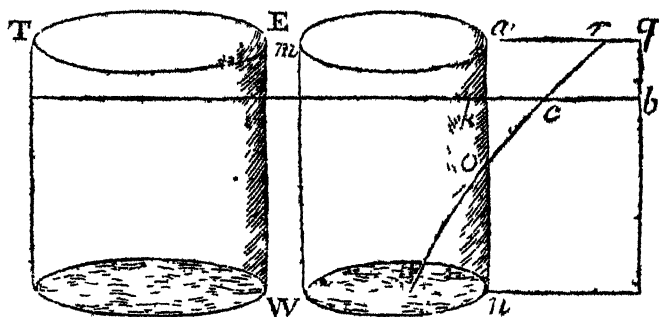
and  $AR=ar$ , may represent the excess of the atmosphere's weight above the pillar of water, the resistance to the pistons from the remainder of steam, and the effective force, respectively, *e.g.* at the beginning of the descent. Take, then, every-where  $ak:AK::an:AN$ , and at all similar positions the resistance  $bc$  of  $mn$  and force  $kc$  on its piston will equal the resistance  $BC$  of  $MN$  and force  $K$  on its piston; and by what Sir Isaac Newton has demonstrated (*Book 1. Prop. 39*) of the descent of bodies, we have  $\sqrt{akcr}:\sqrt{AKCR}::$  celerity in  $k$ : celerity in  $K$ . But these areas being evidently as the corresponding parallelograms  $kq$  and  $KQ$ , and they again as their heights, the celerities generated are in the subduplicate ratio of  $ak:AK$ , as tho' the resistance had been nothing; and by an obvious enough reasoning from the said proposition, the times also appear to be in the above-mention'd ratio; which ratio is not any way varied, tho' the resistance prevails from the intersecting points  $O$ .

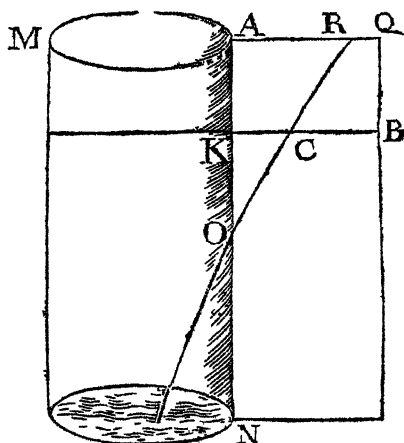
Now, to apply what has been said to the business in hand; if  $TW$  be a cylinder of equal content with the cylinder  $MN$ , the quantity of water delivered by both will, as a consequence of the fundamental law of mechanics observed above, be the same at each lift: but the cylinder  $TW$  is no higher than  $nm$ , and *ex hypoth.* their rarefactions are equal; and therefore, by what has been proved with regard to the times, the time of the piston's descent in  $TW$  will be to that of the piston's descent in  $MN::\sqrt{EW}:\sqrt{AN}$ ; whence in any given time the broad cylinder  $TW$  will perform more than the longer one  $MN$  of equal content, and that in the ratio of their diameters;

for  $\overline{ET}^2 \times EW = \overline{MA}^2 \times AN$ , *ex hypoth.*  $EW:AN :: \overline{MA}^2:\overline{ET}^2$ , and consequently  $\sqrt{EW}:\sqrt{AN}:: MA:ET$ . The friction too is diminished with the slowness of the motion, and because the periphery increases in a less ratio than does the area of a circle.

The result of the whole then is in favour of the broad cylinder ; and still the broader the better ; for unless some mechanical considerations should limit the problem, 'tis evident in a geometrical sense, that there is no limitation. A disadvantage might arise perhaps to the effect of the jet d'eau from thus increasing the breadth ; which however would be remedied, I think, by a number of these jets : but be that as it will, 'tis certain, that to augment the diameters, and diminish the lengths of the smaller kind of cylinders, now used, could have no such inconvenience, nor fail of being attended with an augmentation of force.

What I think might be further observed for the improvement of this engine is in the boiler and steam, but more connected with experiments, which should I have an opportunity to make, I may resume perhaps the subject, if they answer my expectation.





XXX. *Mr. John Bradley's Observation of the Occultation of Venus by the Moon; communicated by Mr. James Short, F.R.S.*

Read June 6.  
1751.

**M**R. Gael Morris having favour'd me with the observation of the late occultation of Venus by the moon, taken at Greenwich with great exactness by Mr John Bradley, I am induced to lay the same before the Royal Society, in order to shew its very near agreement with those phases, which Dr. Bevis observed at my house in Surry-street, allowing for the difference of meridians. I must take notice, that, besides the advantage of a fix-foot reflector with a great magnifying power, which shew'd the planet's limb very well defined, he had also another, which the doctor had not, I mean

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a very serene air, free from smoke, which enabled him to discern and keep sight of the moon during the whole occultation, so that he might observe the moment of the emersion with the same certainty, as that of the immersion: for Mr. Canton, with a reflector of 18 inches only, that day plainly saw the moon at his house in Spital-fields.

### The Greenwich Observation.

Apparent time.	<sup>h</sup>	<sup>'</sup>	<sup>''</sup>	
1751 April 15,	22	41	45	The first contact; doubtful to 1 second.
		42	18	Quite immersed.
	23	15	36 $\frac{1}{2}$	Began to emerge.
		16	8 $\frac{1}{2}$	Wholly emerged.
16,	1	39	12	Venus passed the meridian.

J. Short.

XXXI. *An Account of Mr. Benjamin Franklin's Treatise, lately published, intitled, Experiments and Observations on Electricity, made at Philadelphia in America; by Wm. Watson, F. R. S.*

Read June 6. 1751. **M**R. Franklin's treatise, lately presented to the Royal Society, consists of four letters to his correspondent in England, and of another

other part intituled “ Opinions and conjectures concerning the properties and effects of the electrical matter arising from experiments and observations.”

The four letters, the last of which contains a new hypothesis for explaining the several phenomena of thunder-gusts, have either in the whole or in part been before communicated to the Royal Society. It remains therefore, that I now only lay before the Society an account of the latter part of this treatise, as well as that of a letter intended to be added thereto by the author, but which arrived too late for publication with it, and was therefore communicated to the Society by our worthy brother Mr. Peter Collinson.

This ingenious author, from a great variety of curious and well-adapted experiments, is of opinion, that the electrical matter consists of particles extremely subtil; since it can permeate common matter, even the densest metals, with such ease and freedom, as not to receive any perceptible resistance: and that if any one should doubt, whether the electrical matter passes through the substance of bodies, or only over and along their surfaces, a shock from an electrified large glass jar, taken through his own body, will probably convince him.

Electrical matter, according to our author, differs from common matter in this, that the parts of the latter mutually attract, and those of the former mutually repel, each other; hence the divergency in a stream of electrified effluvia §: but that, tho’ the

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particles

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§ As the electric stream is observed to diverge very little, when the experiment is made *in vacuo*, this appearance is more owing  
to



particles of electrical matter do repel each other, they are strongly attracted by all other matter.

From these three things, *viz.* the extreme subtilty of the electrical matter, the mutual repulsion of its parts, and the strong attraction between them and other matter, arises this effect, that when a quantity of electrical matter is applied to a mass of common matter of any bigness or length within our observation (which has not already got its quantity) it is immediately and equally diffused thro' the whole.

Thus common matter is a kind of sponge to the electrical fluid; and as a sponge would receive no water, if the parts of water were not smaller than the pores of the sponge; and even then but slowly, if there was not a mutual attraction between those parts and the parts of the sponge; and would still imbibe it faster, if the mutual attraction among the parts of the water did not impede, some force being required to separate them; and fastest, if, instead of attraction, there were a mutual repulsion among those parts, which would act in conjunction with the attraction of the sponge: so is the case between the electrical and common matter. In common matter indeed there is generally as much of the electrical as it will contain within its substance: if more is added, it lies without upon the surface ||, and forms what we call  
an

to the resistance of the atmosphere, than to any natural tendency in the electricity itself. *W. W.*

|| The author of this account is of opinion, that what is here added, lies not only without upon the surface, but penetrates with the same degree of density the whole mass of common matter, upon which it is directed.

an electrical atmosphere; and then the body is said to be electrified.

'Tis supposed, that all kinds of common matter do not attract and retain the electrical with equal force, for reasons to be given hereafter; and that those called electrics *per se*, as glass, &c. attract and retain it the strongest, and contain the greatest quantity.

We know, that the electrical fluid is in common matter, because we can pump it out by the globe or tube; and that common matter has near as much as it can contain; because, when we add a little more to any portion of it, the additional quantity does not enter, but forms an electrical atmosphere: and we know, that common matter has not (generally) more than it can contain; otherwise all loose portions of it would repel each other, as they constantly do when they have electric atmospheres.

The form of the electrical atmosphere is that of the body, which it surrounds. This shape may be render'd visible in a still air, by raising a smoke from dry resin dropp'd into a hot tea spoon under the electrified body, which will be attracted and spread itself equally on all sides, covering and concealing the body. And this form it takes, because it is attracted by all parts of the surface of the body, though it cannot enter the substance already replete. Without this attraction it would not remain round the body, but be dissipated in the air.

The atmosphere of electrical particles surrounding an electrified sphere is not more disposed to leave it, or more easily drawn off from any one part of the sphere than from another, because it is equally attracted by every part. But that is not the case with  
bodies

bodies of any other figure. From a cube it is more easily drawn at the corners than at the plane sides, and so from the angles of a body of any other form, and still most easily from the angle that is most acute; and for this reason points have a property of drawing on, as well as throwing off the electrical fluid, at greater distances than blunt bodies can.

From various experiments recited in our author's treatise, to which the curious may have recourse, the preceding observations are deduced. You will observe how much they coincide with and support those which I some time since communicated to the Society upon the same subject.

To give even the shortest account of all the experiments contained in Mr. Franklin's book, would exceed greatly the time allowed for these purposes by the Royal Society: I shall content myself therefore with laying a few of the most singular ones before you.

The effects of lightning, and those of electricity, appear very similar. Lightning has often been known to strike people blind. A pigeon, struck dead to appearance by the electrical shock, recovering life, drooped several days, eat nothing, tho' crumbs were thrown to it, but declined and died. Mr. Franklin did not think of its being deprived of sight; but afterwards a pullet, struck dead in like manner, being recovered by repeatedly blowing into its lungs, when set down on the floor, ran headlong against the wall, and on examination appeared perfectly blind: hence he concluded, that the pigeon also had been absolutely blinded by the shock. From this observation we should be extremely cautious, how in electrifying we  
draw

draw the strokes, especially in making the experiment of Leyden, from the eyes, or even from the parts near them.

Some time since it was imagined, that deafness had been relieved by electrifying the patient, by drawing the snaps from the ears, and by making him undergo the electrical commotion in the same manner. If hereafter this remedy should be fantastically applied to the eyes in this manner to restore dimness of sight, I should not wonder, if perfect blindness were the consequence of the experiment.

By a very ingenious experiment our author endeavours to evince the impossibility of success, in the experiments proposed by others of drawing forth the effluvia of non-electrics, cinamon, for instance, and by mixing them with the electrical fluid, to convey them with that into a person electrified: and our author thinks, that tho' the effluvia of cinamon and the electrical fluid should mix within the globe, they would never come out together through the pores of the glass, and thus be conveyed to the prime conductor; for he thinks, that the electrical fluid itself cannot come through, and that the prime conductor is always supplied from the cushion, and this last from the floor. Besides, when the globe is filled with cinamon, or other non-electrics, no electricity can be obtained from its outer surface, for the reasons before laid down. He has tried another way, which he thought more likely to obtain a mixture of the electrical and other effluvia together, if such a mixture had been possible. He placed a glass plate under his cushion, to cut off the communication between the cushion and the floor: he then brought a  
small

small chain from the cushion into a glass of oil of turpentine, and carried another chain from the oil of turpentine to the floor, taking care, that the chain from the cushion to the glass touched no part of the frame of the machine. Another chain was fixed to the prime conductor, and held in the hand of a person to be electrified. The ends of the two chains in the glass were near an inch from each other, the oil of turpentine between. Now the globe being turned could draw no fire from the floor through the machine, the communication that way being cut off by the thick glass plate under the cushion: it must then draw it through the chains, whose ends were dipp'd in the oil of turpentine. And as the oil of turpentine being in some degree an electric *per se*, would not conduct what came up from the floor, the electricity was obliged to jump from the end of one chain to the end of the other, which he could see in large sparks; and thus it had a fair opportunity of seizing of the finest particles of the oil in its passage, and carrying them off with it: but no such effect followed, nor could he perceive the least difference in the smell of the electrical effluvia thus collected, from what it had when collected otherwise; nor does it otherwise affect the body of the person electrified. He likewise put into a phial, instead of water, a strong purging liquid, and then charged the phial, and took repeated shocks from it; in which case every particle of the electrical fluid must, before it went through his body, have first gone thro' the liquid, when the phial is charging, and returned through it when discharging; yet no other effect followed than if the phial had been charged with water.

He

He has also smelt the electrical fire, when drawn thro' gold, silver, copper, lead, iron, wood, and the human body, and could perceive no difference; the odour being always the same, where the spark does not burn what it strikes; and therefore he imagines, that it does not take that smell from any quality of the bodies it passes through. There was no abridging this experiment, which I think very well conceived, and as well conducted, in a manner to make it intelligible; and therefore I have laid the author's words nearly before you.

As Mr. Franklin, in a letter to Mr. Collinson some time since, mentioned his intending to try the power of a very strong electrical shock upon a turkey, I desired Mr. Collinson to let Mr. Franklin know, that I should be glad to be acquainted with the result of that experiment. He accordingly has been so very obliging as to send an account of it, which is to the following purpose. He made first several experiments on fowls, and found, that two large thin glass jars gilt, holding each about 6 gallons, and such as I mentioned I had employed in the last paper I laid before you upon this subject, were sufficient, when fully charged, to kill common hens outright; but the turkeys, though thrown into violent convulsions, and then, lying as dead for some minutes, would recover in less than a quarter of an hour. However, having added three other such to the former two, though not fully charged, he killed a turkey of about ten pounds weight, and believes that they would have killed a much larger. He conceited, as himself says, that the birds kill'd in this manner eat uncommonly tender.

In making these experiments, he found, that a man could, without great detriment, bear a much greater shock than he imagined: for he inadvertently received the stroke of two of these jars through his arms and body, when they were very near fully charged. It seemed to him an universal blow throughout the body from head to foot, and was followed by a violent quick trembling in the trunk, which went gradually off in a few seconds. It was some minutes before he could recollect his thoughts, so as to know what was the matter; for he did not see the flash, tho' his eye was on the spot of the prime conductor, from whence it struck the back of his hand; nor did he hear the crack, tho' the bystanders said it was a loud one; nor did he particularly feel the stroke on his hand, tho' he afterwards found it had raised a swelling there of the bigness of half a swan-shot, or pistol-bullet. His arms and the back of his neck felt somewhat numbed the remainder of the evening, and his breast was sore for a week after, as if it had been bruised. From this experiment may be seen the danger, even under the greatest caution, to the operator, when making these experiments with large jars; for it is not to be doubted, but that several of these fully charged would as certainly, by increasing them, in proportion to the size, kill a man, as they before did the turkey.

Upon the whole, Mr. Franklin appears in the work before us to be a very able and ingenious man; that he has a head to conceive, and a hand to carry into execution, whatever he thinks may conduce to enlighten the subject-matter, of which he is treating: and altho' there are in this work some few opinions,  
in

in which I cannot perfectly agree with him, I think scarce any body is better acquainted with the subject of electricity than himself.

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XXXII. *A Letter to the Rev. Dr. Hales, F.R.S. from Captain Henry Ellis, F.R.S. dated Jan. 7, 1750-51, at Cape Monte Africa, Ship Earl of Hallifax.*

S I R,

Read June 13. 1751. **I** MAKE use of this opportunity of writing to you, less from the vanity I have of having such a correspondent, than the desire of contributing to his satisfaction, who esteems it his greatest happiness to promote the interest of mankind. At yours and Lord Hallifax's recommendation, I had your ventilators fixed on board of my ship, at Bristol. The following is a detail of the experiments, which I made to prove their utility.

1. I took a wax-candle, of eight to the pound, and drew it thro' a mold, to make it of one thickness from end to end: then weighed it exactly, and lighted it in the ship's hold; where I found it wasted 67 grains in 30 minutes; that place not being ventilated during 24 hours: but after six hours ventilation it wasted  $94 + \frac{1}{2}$  grains in the same time.

2. I carried with me into the hold a plate of silver, well polished, and a lantern and candle, all blinded, except a round hole of about two inches diameter. I placed the plate at six feet distance from it; and with



such obliquity, that the rays from the light should fall on its surface at an angle of 45 degrees. I then fixed a white paper screen, at the same distance from the plate, and under the same angle with the lantern, so that the reflected rays might fall upon it also. This being done, I observed, that the reflection from the plate distinctly was but 17' 30" with an unventilated hold; it being turned the colour of tarnished lead; whereas, when the air was replaced by 4 hours ventilation, it continued to reflect light, and retain its brightness 4 hours 47 minutes.

3. The ship's bell, whose diameter is 14 inches, I had brought into the hold, when ventilation had been omitted 12 hours. Having hung it under the lower deck, I took out the clapper; and having suspended it also by thread, which, with its own length, made 44 inches; the angle, which the rim of the bell made, with a line let fall perpendicular from the pin, on which the clapper hung, was equal to 34' 0". I then held the clapper at the same angle, on the other side of the line, in order that the strokes at different times might be with the same force; when, letting it go, it struck the bell. In its return I caught it, and counting the vibrations, I heard them distinctly but three times; whereas, when the hold was well ventilated, it vibrated five times; but its vibrations were not so quick in the latter, as in the former case. I took all possible precautions, that these experiments might be fairly tried, to prevent deception; but always found them to produce the same effects.

We are at present very healthy, tho' our number is 130, not one being sick aboard. Our hold, which  
in

in most ships is very moist, in ours is quite dry. Our cargo arms, which are kept there in upright chests, without wrappers, come out as bright as from a recent polish. Far is a ventilator from being inconvenient aboard of us; on the contrary, 'tis good exercise for our slaves, and a means of preserving our cargo and lives.

Upon the passage, I made several trials, with the bucket fathometer, in latitude  $25^{\circ}-13'$  north; longitude  $25-12'$  west. I charged it, and let it down to different depths, from 360 feet to 5346 feet, when I discovered, by a small thermometer of Fahrenheit's, made by Mr. Bird, which went down in it, that the cold increased regularly, in proportion to the depth, till it descended to 3900 feet: from whence the mercury in the thermometer came up at 53 degrees; and tho' I afterwards sunk it to the depth of 5346 feet, that is a mile and 66 feet, it came up no lower. The warmth of the water upon the surface, and that of the air, was at that time by the thermometer 84 degrees. I doubt not but that the water was a degree or two colder, when it enter'd the bucket, at the greatest depth, but in coming up had acquired some warmth; for I found, that the water, which came up in the bucket, having stood 43 minutes in the air (the time of winding it up) the mercury rose above 5 degrees. When the air had rendered it equally warm with the water on the surface, I tried then over 100, by weighing equal quantities very exactly, a small hydrometer, and found from great depths the heaviest, and consequently the saltest water.

This

This experiment, which seem'd at first but mere food for curiosity, became in the interim very useful to us. By its means we supplied our cold bath, and cooled our wines or water at pleasure; which is vastly agreeable to us in this burning climate.

I intend, in our passage to the West Indies, to sound a mile deeper than I have done, having a sufficient quantity of line. But I cannot attempt your method to find the depth of the sea, for want of apparatus. My business at present affords me very little time for speculation. However, I cannot omit observing to you a phænomenon, which I saw last night, and never before, that I remember; and that was the two arches of the iris, with their colours distinct, by moon-light. Having already presumed much on your patience, and my leisure,

I am, &c.

Hen. Ellis.

*A Letter to the President, from Stephen Hales, D. D. & F. R. S.*

S I R,

Teddington, June 8, 1751.

Read June 13, 1751. **I** HAVE here inclosed, at his desire, a copy of a letter from Captain Ellis, who published an account of his voyage to Hudson's Bay.

The bucket sea-gage, which he mentions, and which I provided for him, to find the different degrees of coolness and saltness of the sea, at different depths,

depths, was a common household pail or bucket, with two heads in it; which heads had each a round hole in the middle, near four inches diameter, which were cover'd with valves which open'd upwards; and that they might both open and shut together, there was a small iron rod fixed to the upper part of the lower valve, and at the other end to the under part of the upper valve: so that, as the bucket descended with its sinking weight into the sea, both the valves open'd by the force of the water, which had by that means a free passage thro' the bucket. But when the bucket was drawn up, then both the valves were shut by the force of the water at the upper part of the bucket: by which means the bucket was brought up full of the lowest sea-water, to which it had descended.

When the bucket was drawn up, the hole at the bottom was stopped with a cork, to keep the water in, when the valves were open'd, to come at the mercurial thermometer, which being tied to an upright stick, could readily be unfastened, by pulling out a loose nail, which went into the upper end of stick, which was fasten'd at its lower end in the same manner.

But great care must be taken to make an observation of the degree the mercury stands at, before the lower part of the thermometer is taken out of the water; else it would immediately be alter'd by the different temperature of the air.

In order to keep the bucket in a right position, there are four cords fixed to it, which reach about three feet below it, to which the sinking weight is to be fixed.

Captain

Captain Robinson, who is lately arrived from India, says, he found so much benefit by ventilators, that he will never go a voyage without them; and that he lost but two men in two years.

There are many other instances of the benefit of ventilators in ships, not only to the health and lives, but also to the provisions, &c.

I am, Sir, with great respect,

Your obliged humble servant,

Stephen Hales.

XXXIII. *Observations on the Roman Colonies and Stations in Cheshire and Lancashire, by Thomas Percival Esq; communicated by Hugh Lord Willoughby of Parham, F. R. S.*

Read June 13. 1751. **I**N the second iter of Antonine's Itinerary, we find, after several other stations mentioned Eboracum

Calcariam M. P. IX.

Camulodunum M. P. XX. Tho' with various

Mamucium M. P. XVIII. readings of the

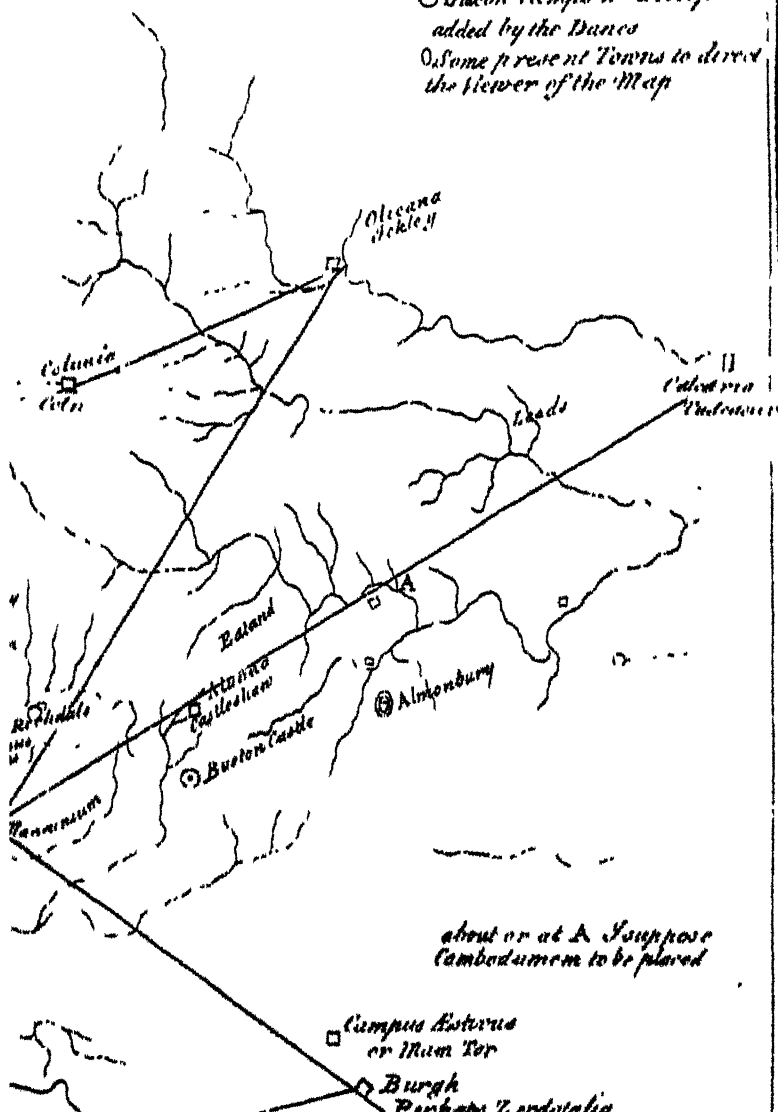
Condate M. P. XVIII. names.

Devam M. P. XX.

It is agreed, that Deva is Chester, and that Mamucium or Manucium or Mancunium, is Manchester, by the common consent of all antiquarians. But where Con-  
date



- Roman Camps
  - Saxon Camps, Castles
  - Saxon Camps with a Keep
- added by the Dances
- On some present Towns to direct  
the Viewer of the Map









date is situated, is yet a matter of debate. Some (as Mr. Camden and others) declare for Congleton; some also for Northwich; but I think equally wrong. But to understand me rightly, be pleased to lay before you Gibson's Camden, vol. i. and in the map for Cheshire you will observe Stretford in Lancashire. Here, it is certain, the Roman road passed the Mersey, as well by the name, as the visible remains in the meadows near the present bridge. About a quarter of a mile from Altringham you see the road very plain, as also near Dunham and in Dunham park. More southerly you see Chapel in the Street; an evident mark of the Roman way having gone near it. By this course it is evident Congleton cannot be the place; the course of the road leaving it too much to the east by several miles; and laying a ruler over the map, you will perceive the Roman road proceeded in a direct line from Stretford to Dunham-park, leaving Altringham to the east, and so directly forward till past Rosthern Mere, where it must have made an angle \* to go to Chapel in the Street: continuing which line forward to the south, you will find, that it points to Kinderton, the situation of which is between a river and a brook, and remains of the road may be seen to the west of Rudheath, now called Kindstreet, and a square Roman camp on the Lingula to the west of Kinderton. Thus the name of Congleton, which induced Mr. Camden to place Condote there, agrees less with the name than Kinderton. The common characteristic of Agricola's station agrees with

E e                      Kinderton;

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\* See the map annexed.

Kinderton; it being on a Lingula, which Congleton is not. A Roman camp, which I am well assured by a friend to be there, marks the very place, as the pointing of the Roman road confirms the opinion. And in the tenth Iter, Condute being placed in the road to Mediolanum shews it to be easterly of Chester. I say, all these reasons confirm me in a belief, that Condute is Kinderton. Mr. Horsley observes, that Condute signifies the confluence of two rivers; a situation, which Kinderton has. I now turn back to Manchester.

Mancunium is agreed to be Manchester. The Roman fort is at Knotmills, and stands on a high piece of ground overlooking the confluence of Irwell and Medlock, but nearer the Medlock, that river running within about 60 yards of the fort. The fort is square, and has been surrounded with a wall. The whole fort is 6 or 7 feet higher than the rest of the hill; and the whole strongly cemented with mortar. The Medlock runs upon, or rather forces its way thro', a rock under it. So that, from the situation, as well as strength, it well deserved the name of Mancunium; in British *Maen Cune*, i. e. the stone city.

The Roman road from Mancunium to Eboracum or York goes near the top of the Deansgate in Manchester, and crossing the inclosures on the south-east end of the town appears in an inclosure near Ancoats; then runs thro' Bradford, and crosses the very middle of Newton-heath, Newton-chapel standing on the very ridge of it. Standing at the west end of the chapel, you see the trace of it into Bradford-lane; standing at the east end, you see the trace of it go betwixt a house

a house and a barn on the east end of the common. It then runs thro' the inclosures to Mr. Wagstaffe's house, where it enters a lane, and is visible enough. In about 400 yards more, being interrupted with a moss, it rises with a prodigious grandeur, and is the finest remain of a Roman road in England, that I ever saw. This is at the back of Mr. Jenkinson's house in Failsworth, his land lying on both sides, and is now called Street. It is visible for half a mile more along a back lane leading to Hollinwood, but on the lane turning to the common it strikes cross a meadow of Mr. Whitehead's, and is visible for some small part of it. Tradition directs its course to Glodwicklows; and some places, where it has been found in ploughing, shew its course to be so. And near Glodwick it is visible in a meadow for some scores of yards pointing over the lows. Tracing it forwards it is very visible at the descent of the hill quite over Mr. James Wyld's land.

There is a small cob on this hill by some supposed to have been a fort: if it was, it must have been a very small one; tho' I rather take it for a *tumulus* than an exploratory tower.

It crosses hence, and is very visible in the grounds of John Mayol, of Wellihole. It then goes thro' the Rev. Mr. Townson's land, leaving Heigh-chapel a little to the south, and so goes up the hill to \* Osterlands on the upper side of the village making towards the Highmore; and going along the inclosures on the south edge of it comes close to Knothill in  
E e 2      Saddleworth;

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\* Here it enters Yorkshire.

Saddleworth, and along the side of Knot-lane, and so crosses over the present road from Manchester to Huthersfield at Delf, and goes over the fields to Castleshaw.

At Castleshaw I was well pleased to find a double Roman camp, and on looking into Ravennas's geography to find between Mantio and Camboduno the name of Alunna, which, in my opinion, is the name of this camp.

It was absolutely necessary for the Romans to have a camp hereabout, considering it was the main pass over the hills and the distance about a Roman march: that from Castleshaw to Manchester is reckon'd ten miles, and the camp is about half a mile beyond; but as the present road is two miles about, it will be about 9 computed miles, and lying at the very foot of the greater ridge of hills, was a proper resting-place on their marches.

From the camp looking toward Manchester, on the top of Knotthill, you see on the very top a very conspicuous Roman *tumulus*; but of this more anon.

From Alunna or Castleshaw the Roman way goes directly for the hill called Clowze-mofs, where it was cut thro' the mofs, and is called Old Gate, being visible by the greenness of its tract; so over the top of Clows or Clowze-mofs. It is visible in a green tract over the Reaps (a hill so called) leaving March-hill or Matthill a little to the north, and Marsden about a mile and a half to the south, pointing directly on Pole-moor, going in its way over the middle of of Holm-moor, and so directly up Cupwith-moor to Polemoorstone, or Guide-post, above Slaighwait or Claighwait, and along the north end of Gowkerhill

or Wholestonesmoor, or Hoolstonesmoor, leaving the rocking-stone about 500 yards to the south.

Standing at Polemoorstone,

the Roman way is west and by south ;

Gowkerhill-end east and by north ;

and Almondbury east-south east ;

horizontal distance three miles and a half,

computed 5 miles.

So that it is plain, that Almondbury was not the Roman station, and Greatlandmoor is at least a mile wide to the north of the track of the road. The road then makes for Lindleymoor, where it is visible for about a mile on the side, and points full towards Tadcaster, Almondbury, and Greatland, both wide, one 2 miles to the north, the other 4 miles to the south.

The great question, where Cambodunum is situated, whether, according to Mr. Camden, at Almondbury, or, according to Mr. Horsley, at Greatlandmoor, may be so far determin'd, that is, at neither. For certainly the road would go strait to the station, or near so. Now it is apparent, that from Manchester to Almondbury the road would have been strait to Castleshaw, but would there have parted from the present track of the Roman road, and gone more south-east by Marsden to Almondbury ; and, as I fancied a road might turn thither, I have made a diligent search for 4 or 5 summers last past ; and living but six miles from Castleshaw, have made all possible inquiry from the shepherds, turf-getters, &c. and of the people at Marsden, whether in ploughing they have met with any remains, but could never yet hear one word of any *via militaris*, or road going  
that

that way. On the contrary, they all speak of the present highway being found out some time since in their grandfathers or great-grandfathers memories; and that the old highway was along the track of the Roman road.

But to turn to the map of Lancashire in Gibson's Camden's Britannia, vol. 2. lay a ruler from the junction of the Medlock and Irwell over Newton, and drawing a line quite beyond Saddleworth, about half an inch on the line, on the east of the river Taume, will be this situation of Alunna or Castleshaw. Note, Saddleworth is not a village, but a large valley, and therefore ill laid down in the map. The church would stand a mile to the south of the line, if that had been rightly placed; tho' Castleshaw is in Saddleworth. Note, a junction of two brooks should be described near Castleshaw, which, when joined in some small distance, fall into the Tame.

Turn now to the map of Yorkshire West-Riding, and laying one edge of your ruler to the junction of 2 small rivers or brooks, you see to the north of Saddleworth. Let the same edge be placed at Rastrick, and a line drawn from one end or the junction of the river to Rastrick will represent the road, as far as I have traced, to within a mile or less of Rastrick. I was in great hopes to have found the station near Gowkerhill, or upon Lindley-common, but was disappointed, and could hear of no camp thereabouts, except one at Kirklees, where there is a large Roman camp, tho' it seems to lie a little too much to the south; unless the road gave a small turn to pass the Calder at some more convenient ford: or, if the Roman road passed the Calder at Brighouse, as I suspect,

suspect, that is not a mile from the camp at Kirklees; and so if Kirklees was not the station, it might be the *campus æstivus* of the station, and the station be on some of the hills, which hereabouts lie close to the Calder. But of this I hope more particularly to search at some convenient opportunity. Only thus far I dare be bold to say, that betwixen Manchester and Lindley-moor are no more Roman camps than Castleshaw: for I have traced almost every foot of it, that is visible, and am certain no camp in that distance could have escaped my view.

It may possibly be asked, why I do not chuse to fix Cambodunum at Castleshaw? I answer, I imagine it too near Manchester; and I should rather think it stood on the military way on the Yorkshire side of the hills, and was intended as a guard to the way on that side, as Castleshaw certainly was on this.

From Castleshaw to Rastrick is 9 computed miles, mostly over the tract of the road, which is to this day used in the summer; and supposing the station to be half a mile on this or that side of the Calder, it will, on Mr. Horsley's calculation of the measures, be about 14 or 15 Roman miles. However this is certain, that the XVIII Roman miles in the Itinerary would, if Mr. Horsley's measures are right, fall nearly on the road near Marchhill or Marshill, which I have searched over and over again, and three computed miles on each side, without finding the least marks of any camp but Castleshaw. March-hill is a fine dry round green hill, too big for a raised *tumulus*; tho' from its appearance one would be apt to think, that it had been a little rounded by art; at  
least



least I doubt it was an encampment of the men, whilst at work on the road, and perhaps a baiting-place on their marches; tho' there are no vestigies of any trench remaining, it being the only place free from moss for some miles, and a fine spring near it.

Imagining with Mr. Horsley, that **xxiii** might be the right number, I searched Gowkerhill-end, and Lindleymoore-side, to no purpose. I therefore imagining, that the distance should be **xxviii**, which will fall nearly on Rastrick; unless you will suppose, that the Roman **xviii** miles are as long as our computed miles, which would still fall (reckoning on the course of the Roman highway) near Rastrick on the river Calder. I could wish such of the gentlemen, who are antiquarians, and live near Rastrick, would inquire of the neighbours thereabouts for the road, or for a camp. For I find it not a little difficult to persuade the country people to give any information, unless they know the inquirer.

Perhaps the names of Castlesteads, Castleshaw, Campfield, or some such other name, may yet remain to guide an antiquarian to the place, as the name of Castleshaw was the guide to me to find out the station, which I suppose to be Alunna.

But to speak more intelligibly to the point: from Eboracum to Calcaria being **ix**, from Calcaria to Cambodunum being **xx** miles, and to Mancunium **xviii**, in the Itinerary; it must be consider'd, that from York to Tadcaster is **9** computed miles, answering **ix** in the Itinerary. From Tadcaster to Rastrick is **20** computed miles; and from Rastrick to the fort at Manchester is along the track of the Roman road **18** computed miles. So that if the  
Romans

Romans gave as long measure in the north, as we now do (and they must, if the numbers of the Itinerary are right) then Cambodunum must be situated near Rastrick on the banks of the Calder. As therefore the numbers in the Itinerary agree not with the true distance of Tadcaster and Manchester, unless the Romans reckon'd their miles, as above observed, which is contrary to the received opinion; and as it would make a very great difference in the sum total of the second *iter* to add with Mr. Horsley one third to our computed miles, we must be reduced to the dilemma of allowing the numbers either to be wrong in the total, or that the miles of the Itinerary are not equally exact.

Here I beg leave to observe, that Mr. Horsley, in accounting for the difference, says, the road being very level betwixt York and Tadcaster, and betwixt Manchester and Chester, if the horizontal miles are the miles meant, the difference of the miles betwixt Tadcaster and Manchester may be accounted for, by the ground being mountainous. To obviate this, observe, that from Manchester to Castlethaw the road is straight, and but two hills in the way, about as high as Highgate-hill. From Castlethaw it goes up a constant tho' moderate ascent for 2 miles; then a gentle descent for a miles; then a gentle descent for a mile to Marshill; then over a small moor and a small valley, and then rises for 2 miles a gentle ascent, and then goes down to Rastrick a gentle descent for 4 miles more. So that had the Romans searched all our moors over, they could not have found a way over, less intercepted with mountains and valleys, rocks and rivers, than this.

I beg leave to observe, that as I find a Roman camp at Castleshaw at the foot of the hills, so in all probability there were other camps betwixt the stations. And I question little, but that they might have one between Calcaria and Cambodunum, possibly at or near Leeds; another between Mancunium and Condate, possibly near Dunham-park; and one between Condate and Deva, perhaps near Chamber in the forest; tho' as these were not settled stations or constant garisons, they may not occur, nor indeed was there any necessity for their occurring, in the Itinerary, as in summer the army might march through, tho' perhaps not in the winter. Yet this I am fully of opinion of, that Castleshaw must have been a settled garison, at least in the time of war; the situation for command of the road, the vicinity of the mountains, all requiring one to render the ways secure. And it is so situated, that a man or centry from Clowzemoss commands a prospect to Manchester, and sees most of the course of the Roman way, and also into Yorkshire, as far as Lindley Moor: as also a man or centry on Knothill might easily see to Manchester, and quite up the hill to the top of Clowzemoss. So that if a centry or small guard was placed at Lindley Moor, another on Clowzemoss, another on Knothill, in time of war, no enemy could march along the course of the way on either side the hills, but notice might be communicated by fires, smoaks, or otherwise, time enough to alarm the garisons.

Give me leave now to turn to the 10th *iter*, and to that part of it, which says,

\* Valacum

Bremontonacis	M. P.	xxvii	al.	xxxii	Thus Mr.
Noccio	.	.	xx	. xxv	Horsley cor-
Mancunio	.	.	xvii	. xxvii	rects them.
Condate	.	.	xviii	.	.

Mr. Horsley says, that the Roman way is not known betwixt Overborrow and Manchester. Bremontonacis is agreed to be Overborrow, and the military way is very visible in several places, as I have myself seen; but take the words of Mr. Rothmell.

“ The Roman way begins at the fortrefs of Ribchester, and runs north over Longridge-fell, and discovers itself by being green, when the rest of Longridge is heathy and morassy on both sides the way; upon which account it is called Green-lane. As soon as it reaches the north summit of Longridge, it makes a right angle, and runs on the north side of the hill towards the east. And after some length it turns by degrees to the north, and then points directly towards Overborrow. It enters Yorkshire a little below Dowford-bridge, and proceeds in a direct line by Newton and Slaitburn to Cross of Greet. It is very apparent on the north side of Tatham-chapel. It runs thro’ Bentham to Overborrow; but the improved country short of Overborrow has eradicated it. It was open’d, on the ground being improv’d, near Dowford-bridge, and was paved 7 yards broad.”

Now, as this proves, that there is a Roman highway betwixt Ribchester and Overborrow, so the Roman highway betwixt Manchester and Ribchester is well known.

From the fort at Manchester it goes along the Dean’sgate by the old church down the Huntsbank,

and so by Strangeways. It is visible in the foot-road to Kearsal-moor, and called the Devil's causeway. It goes near Prestwick church, leaving a *campus agstinus*, now called How-castle-hill, about 20 rods to the right. It goes thro' Radclyffe, and so over Cocky-moor; and from thence to Offey-side to a place now called Watlingstreet; and so to Bellthorn-moor above Darwen, and on the east of Blackburn strait to Ribchester. From Manchester to Ribchester is called 20 miles thro' Blackburn; but the road now gone is certainly longer by 2 miles than the course of the Roman road; which to be sure is about 18 computed miles.

The distance between Ribchester and Overborough, is, I suppose, (considering the angle made on Longridge-fell, and another to get over the valley near Cross of Greet) about 20 computed miles.

At Ribchester there are visible remains of a Roman highway crossing Watlingstreet (*i. e.* the road of *iter*) the eastern branch of which comes from Ickley to Coln, and so by Whalley to Ribchester. Ickley is agreed to be Olicana. Coln, by the name, the *via militaris*, and Roman antiquities, appears to be Colunia; as Whalley for the same reasons must be Gallunia. The western branch of the way goes over Preston-moor, leaving the town above half a mile on the left, and proceeds direct for the sea. I have not had an opportunity to trace it thither; but I doubt not but it leads to the antient *portus Setantiorum*.

A military way goes also from Ribchester to Lancaster, the Longovicarium of the Romans; another from Overborough to Lancaster. Near Overborough is a *castrum exploratorium* on the top of Ingleborough-hill.

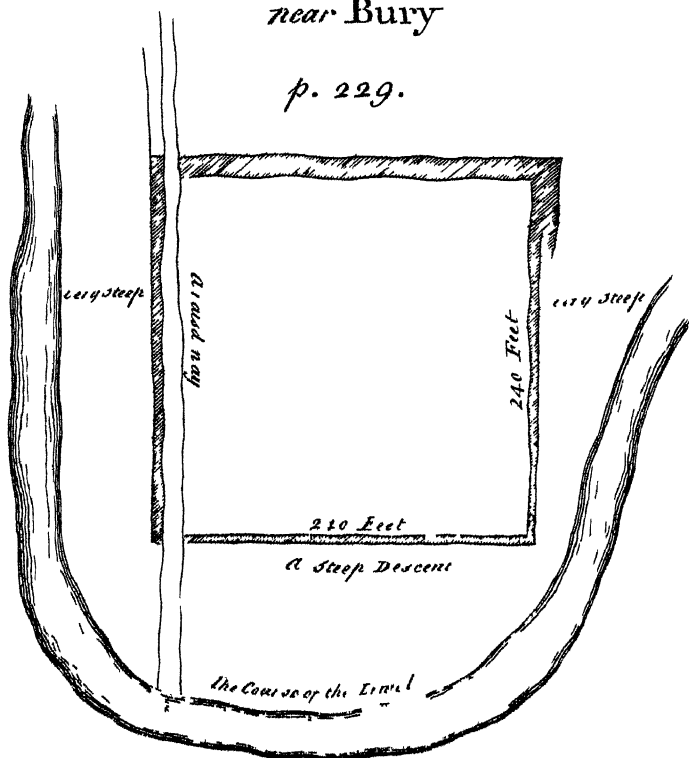


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N.<sup>o</sup> 4.

Castle Steads in Walmisley  
near Bury

p. 229.

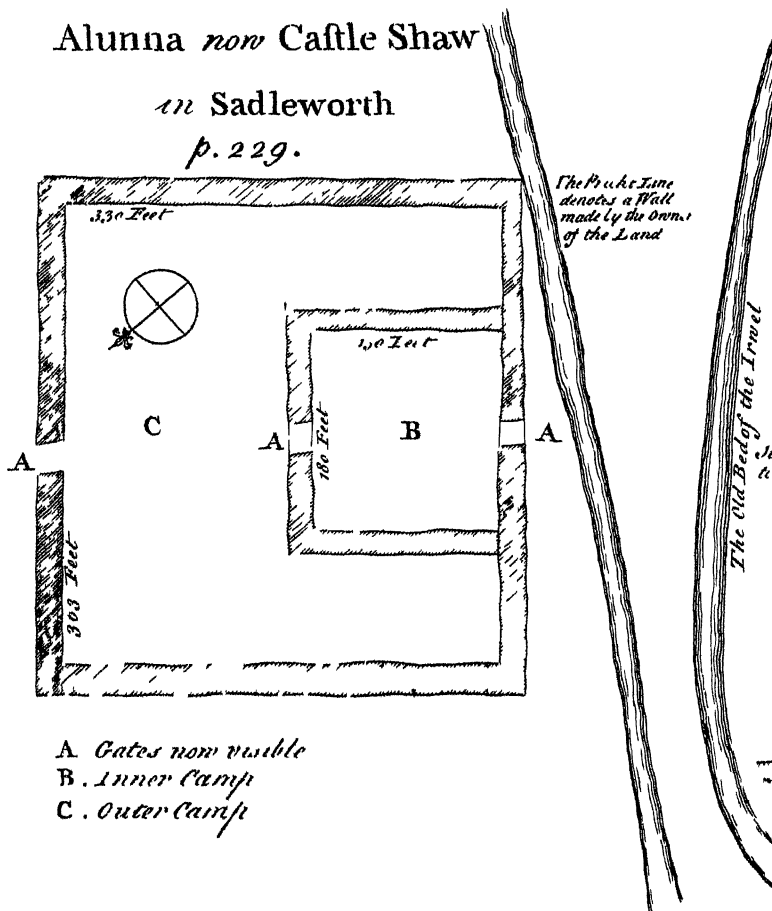


N<sup>o</sup> 5.

Alunna now Castle Shaw

in Saddleworth

p. 229.







hill. A military way goes from Overborrow easterly towards Aferig. The road of the *iter* coming from the north is yet visible. I am of Mr. Camden's opinion, that about Cockey-moor should be placed Coccium. I search'd along the course of the Roman way for a camp without success. However at Bury, about a mile out of the course of the way, is a Roman camp, which I take to be Coccium; tho' I cannot account for its being in the Itinerary, unless Ribodunum was then burnt down, and that Coccium being mentioned as the next camp, was stuck in the place, without a due regard to altering the figures.

Bury is a town lying on the Irwell; and on the west side, where the river makes an elbow, is the Roman camp\*.

There is a Roman camp on the same river above it, which I call the *campus æstivus*; the fortification not near so large as Bury †.

The people have a tradition, that the two camps were relative to one another, and that a battle was fought near Bury, and that the army, or one of them, came over Ashworth-moor, where was a castle. On searching Ashworth-moor, I found a circle cut in the earth ‡; which seems more likely to be a druid's *tumulus* (as Dr. Stukeley describes them) or if not that, I know not what.

At Heap, a mile from Bury, is a *tumulus*; and another at Heywood, about a mile distant from the first.

I

\* See the plan, N<sup>o</sup> 4.

§ See ditto, N<sup>o</sup> 5.

‡ See ditto.

I mention'd Knothill to be a Roman *tumulus*. The people about Castlethaw have yet a tradition, that some great man belonging to the castle was buried there, and have a confused notion of a march of an army of Danes.

Now as Canutus marched into Yorkshire out of Lancashire, it is highly probable, that he came over this road : and as Knott-hill gave him a full view of the Yorkshire moors, it was a proper place and opportunity to harangue his men ; and that speech might alter the old name of the *tumulus* to Knot-hill, if it was not made for his use, which, I think, it was not.

Several names of places on this road seem to carry his memory in their names. Knothill here ; Knotty-lane just below ; Knotlanes between here and Manchester, very near the Roman highway ; Knots-mills near Manchester ; and Knutsford in Cheshire, which way he probably came, in his march from Staffordshire.

N. B. I imagine Ravennas's Geography to be a kind of an *iter* ; and that before the name of Manchester the name of Zerdotalia means Burgh near Castleton in the Peak. For a Roman way comes over the moors from Burgh toward Manchester, another from Burgh to Buxton. There is a Roman camp at Burgh, a *campus æstivus* about a mile distance on the top of Mam-tor, and just below this camp is a lead-mine called Woden or Oden Great Mine, reputed the oldest in Derbyshire, and to have been wrought for many ages. What analogy there is between this name, and the name of the Saxon deity Woden, I refer to be consider'd by the curious, and the reason of its being now affixed to the mine.

XXXIV. *An Account of Professor Winkler's Experiments relating to Odours passing through electrified Globes and Tubes, being the Extraēt and Translation from the Latin of two Letters sent by that Gentleman to Cromwell Mortimer, M. D. Secretary of the Royal Society. With an Account of the Result of some Experiments made here with Globes and Tubes, transmitted from Leipfic by Mr. Winkler to the Royal Society, in order to verify the Faēts before-mentioned, by Mr. W. Watſon, F. R. S.*

Read June 20.  
1751.

PROFESSOR Winkler, in his first letter to Dr. Mortimer, dated at Leipſic, March 12, 1748, mentions, among other particulars, that if odoriferous substances were included in glaſs globes and tubes cloſely ſtopped, and if theſe globes were electrified, the ſmell of the odoriferous ſubſtances would as eaſily as the magnetical power paſs through the glaſs, and be conveyed with the electrical effluvia to conſiderable diſtances, upon ſubſtances readily conducting electricity: that when a man was electrified with a globe of this ſort, the odoriferous matter pervaded his whole body; and that not only his ſkin and his cloaths, but his breath, ſaliva, and ſweat, were impregnated with the ſmell of the ſubſtance included in the glaſs. That after theſe globes had been rubbed a few minutes, the flavour

of their contents would be strongly perceptible upon entering the chamber, in which this operation was performing; and that the substances which he had then tried, were sulphur, cinnamon, and balsam of Peru.

Mr. Winkler mentions, that when he made use of sulphur in his globe, in company with his friend Mr. Haubold, and others, the smell of the sulphur was perceived at more than ten feet's distance, and was so prevalent, that his company was driven away thereby: but that himself staying therein some time longer, his cloaths, his body, and his breath, were infected thereby; and that this smell even continued upon him the next day. Moreover upon his repeating this experiment, as he had before found, that sulphur had been useful to him, he on the third day found in his mouth manifest indications of an inflamed blood.

After this he wanted to transmit a pleasant odour; and for this purpose employed cinnamon, which under the like circumstances sent forth its odour in great abundance; so that it was not only immediately perceptible to any one entering the chamber, but continued there the next day.

Balsam of Peru, under the like treatment, so impregnated the air of the room, that the cloaths and the breath of the persons therein smelled of the balsam, after having passed through several streets; and that Mr. Winkler, when drinking his tea next morning, still perceived the flavour thereof. A few days after, when the smell of the chamber was gone off, he conducted a chain upon silk lines from thence through the open air into another chamber quite separate from the former. In this second chamber  
he

he placed a man upon a silk net, who held the chain in his hand, and after having electrified him with the sphere containing balsam of Peru for a quarter of an hour, any person, who was perfectly ignorant of what was doing, would immediately smell the balsam therein. The man, who was electrified, said, that his tea next morning had a finer taste than usual.

As these experiments did not succeed here, though attempted with a due attention to whatever could be imagined necessary thereto; and as they had done so no-where upon the continent, Italy alone excepted, Dr. Mortimer was desired by the Royal Society to acquaint Mr. Winkler of this want of success, and at the same time to desire him to transmit hither, not only a circumstantial account of the manner of making his experiments, but likewise, lest the difference of the result might arise from employing different kinds of glass, some globes and tubes fitted up under his own eye in the most advantageous manner. This Mr. Winkler was so obliging as to comply with; and accordingly the Society has received from him two globes and four tubes; and at the same time this gentleman sent a letter to Dr. Mortimeri dated at Leipzig, Nov. 23, 1750, of which the following is a translation from the Latin of so much as relates to these matters.

“ You desire me, as soon as the grief for the loss  
 “ of my wife would permit me, to explain, in the  
 “ most clear and intelligible manner, my experi-  
 “ ments, whereby spices and balsams, by their fia-  
 “ grance, pervade glass, when electrified. Glass globes  
 “ and tubes ought to have this property, that, when  
 G g “ the

“ the latter are rubbed backwards and forwards  
 “ through the hand, or the former with the hand  
 “ applied thereto, they give manifest tokens of the  
 “ electric power. Moreover the glass of these tubes  
 “ and globes ought to be thin; lest the thickness of  
 “ the glass should prevent the transmittion of the  
 “ odours. It is necessary, that the spices be dry, and  
 “ broken small, and that spirituous liquors, as well  
 “ as the more liquid balsams should be well mixed  
 “ with powder'd chalk. But how great the quantity  
 “ may be, either of the spices, balsams, or spirituous  
 “ liquors, which should be included, cannot be de-  
 “ termined; because it is not yet certain, how much  
 “ of the electrical power is necessary for dissolving  
 “ the odoriferous particles, and carrying them along  
 “ with it. But as the fact itself is manifest, I have  
 “ taken upon me to transmit to the Royal Society, for  
 “ which I have the highest regard, two globes and  
 “ four tubes. I hope, that these tubes, when rubbed  
 “ as usual between the hands furnished with a piece  
 “ of thin and somewhat rough cloth, and that these  
 “ globes, if mounted upon the pillars of an electrical  
 “ machine, and either rubbed with a naked but very  
 “ dry hand, or with a piece of silk or woollen cloth,  
 “ will transmit odours, plainly different from the  
 “ odour of the electric matter, and which persons  
 “ here at Leipzig of good noses have distinctly per-  
 “ ceived. To know indeed this difference, it is ne-  
 “ cessary, that, before the prepared tube is rubbed, a  
 “ tube containing nothing odoriferous be tried; and  
 “ lest the friction should be attended with no effect,  
 “ great care must be taken that the outward surface  
 “ of the globes and tubes be perfectly dry.

“ Of

“ Of the tubes one contains flowers of sulphur  
 “ this was sent me from Dresden by Mr. Haubold,  
 “ mathematician and geographer to the king of Po-  
 “ land. It is the same sort with one, with which  
 “ that gentleman shewed the late Count Saxe the  
 “ penetration of the sulphureous odour, when he  
 “ was last year at Dresden. In another I have in-  
 “ cluded balsam of Peru, mixed with powder'd chalk.  
 “ In the third, opobalsamum; and in the fourth,  
 “ spirit of wine with chalk.

“ The larger globe contains opobalsamum, and  
 “ the smaller beaten cinnamon.

“ In making use of the globe with cinnamon,  
 “ this method is to be observed. After that, from  
 “ the rotation against the hand or a rubber, the  
 “ globe is warmed, let the motion be disconti-  
 “ nued. After this discontinuance, let the hand  
 “ be immediately applied to the globe, and the nose  
 “ of any person, who is willing to make the trial, is  
 “ to be held within an inch or two thereof; and the  
 “ rotation to be repeated by little and little, and to  
 “ be made slowly. In this repeated and gentle ro-  
 “ tation the observer will perceive the agreeable va-  
 “ pour of cinnamon; but this vapour quickly va-  
 “ nishes upon continuing the rotation. It is there-  
 “ fore necessary, that, as soon as the globe is heated  
 “ again, the rotation should be stopped, and be be-  
 “ gun again by little and little, when, upon the first  
 “ turn of the globe, the exhalation of the cinnamon  
 “ will be perceived. And this may be repeated as often  
 “ as you please, only observing, as often as the globe is  
 “ heated, that after a short respite you begin the ro-  
 “ tation of the globe in a very gentle manner.



“ I beg of you, sir, in the most solemn manner, that you would explain these rules to Mr. Watson, and intrcat him, that, when the trials of these globes and tubes shall be made in the presence of several persons, all these circumstances may be regarded; lest any thing be omitted, which may conduce to the knowledge of the truth.”

The tubes and globes referred to in the above letter were received by the Royal Society about the middle of May 1751, and were presented to that body by the President at their next meeting; and they were put into my hands, in order that their effects upon trial might be reported at a future meeting.

The largest sphere was of crystal glass of about seven inches diameter, fixed to its wooden spindles by a resinous cement, and contained not more than half an ounce of a terebinthinate fluid, less deep in colour than balsam of Peru, and more so than balm of Gilead. The smaller globe was five inches in diameter, was mounted nearly as the larger one, and contained about half an ounce of beaten cinnamon. The tube containing the flowers of sulphur was two feet in length, and about half an inch in diameter: it, like the globes and the other tubes, was of crystal glass, and in like manner with the rest of the tubes was hermetically sealed. The tube, said to contain Balsam of Peru and chalk, was about twenty inches long, and  $\frac{3}{4}$  of an inch in diameter: that said to contain opobalsamum was about sixteen inches long, and half an inch in diameter: and that with spirit of wine and chalk was about seventeen inches long, and about half an inch in diameter.

The manner of mounting these globes might be somewhat exceptionable for the purposes intended,

as the necks were fitted to their wooden blocks with a resinous cement without glass stoppers; so that when the globes, from their being rubbed, had warmed the cement, if an odour of the matter contained in the glass had been perceptible, it might have been urged, that it came through the cement with more probability than through the glass: but nothing of this kind could be objected to the tubes, as they were hermetically sealed.

June 12, 1751, there met me at my house, in order to make trial of the effects of these glasses, Martin Folkes Esq; President, Nicholas Mann Esq; Vice-president, Dr. Mortimer and Peter Daval Esq; Secretaries, Mr. Canton, Fellow of the Royal Society; and Mr. Schrader, a gentleman of distinction well known to, and corresponding with Mr. Winkler. The presence of this gentleman was highly agreeable to the company; as he was thereby enabled to satisfy both himself and his friend Mr. Winkler of the zeal and address, which we exerted in order to verify Mr. Winkler's assertions. The weather was dry, and very fit for electrical experiments. Not the least alteration had been made in Mr. Winkler's globes; but as, with its mounting, one of them was too wide to be placed between the posts of my electrical machine, these posts were altered for that purpose.

The largest globe, said to contain opobalsamum, was first put the trial: it was first rubbed a considerable time with a dry hand chalked, and the snaps at the primeconductor were but weak; but upon rubbing the globe, first with the cushion, which I have usually for that purpose employed, and afterwards with red leather, the snaps were much stronger; and Mr. Canton, as well as another gentleman present, were electrified

electrified by turns therewith: but all this while no smell of the balsam could be perceived by any of the company, either upon the equator of the globe, the persons electrified, the prime conductor, or any of the rubbers made use of; though for this purpose we carefully observed, not only the method suggested by Mr. Winkler, but such others, as appeared to us the most conducive to the present purpose. When the globe was heated, indeed by putting our noses near the mounting, we could smell the resin therein; but this was all. We employed quick motion, afterwards we permitted the globe to be still, and then began again with an extremely gentle motion; but still no odour of the balsam could be perceived in the room, though for that purpose a person was called in, well-skilled in these odours, and who, from his coming fresh out of the air, it was suggested might more readily perceive them: but this, he declared, he was not capable of doing.

We next tried the lesser globe containing cinnamon, and most punctually observed Mr. Winkler's directions, as he is more especially exact in what, he thinks, should be observed to make this experiment succeed: but our endeavours were to no purpose, for we could never, after many trials, either smell the cinnamon, or make the electricity the least perceptible upon the prime conductor. This indeed was what I had always heretofore observed, when I endeavoured to make this experiment; as such a quantity of non-electric matter, unless the sides of the globe were very thick, prevented the exciting the electrical power, even when I employed globes much larger than this sent by Mr. Winkler.

We then began with the tubes : As you see by their dimensions, they were, except that containing the sulphur, by much the smallest I had ever seen used for these purposes : but every gentleman has a right to perform his experiments in his own way. Accordingly their power in electrifying was but weak ; for though some of them attracted and repelled the leaf-silver tolerably well, yet when a man was attempted to be electrified with them, the snaps from his hand were very small. Of these four tubes, that with sulphur was much the strongest ; the next to it, that said to contain opobalsamum ; then that with balsam of Peru, and chalk ; but the least of all, that with spirit of wine and chalk, which with the common rubbers scarce attracted the silver ; but when rubbed by some silk prepared with linseed-oil, and brought by Mr. Canton, the attractive power was increased, though even then it was very little. Mr. Canton has for some time usually rubbed his tubes with this oiled silk, which he has found by experience to produce the greatest effects, but he does not think it proportionably useful in rubbing globes. In their turns the globes and all the tubes were rubbed with this oiled silk ; but no one of the company, after very many trials in different ways, could perceive the least odour of the substances contained, either upon the outside of the tubes, or upon the substances electrified thereby.

We thus spent more than two hours without success, in our endeavours to see the effects proposed by Mr. Winkler ; for we were unfortunate enough not to be able to verify them in one single instance.

There

There appears a very great disparity between the two letters from Mr. Winkler to Dr. Mortimer concerning these facts. In the first we are informed, that the effluvia from balsam of Peru were not only perceptible in the person electrified, and in the air of the room; but that these were carried along with the current of electricity through the open air into another chamber: that his company did not chuse to bear the offensive smell of the brimstone transpiring through his glass; and that it even heated his own blood: that cinnamon also sent forth its odour in great abundance, perceptible to any one immediately entering the chamber, and continuing there till next day.

In the second letter you will perceive, that there is a great abatement of what we were promised to expect from the first: we are there told, that the glass globes and tubes now sent, if they are electrified, transmit odours, not those directly of the substances included, but such as are plainly different in smell from the electrical effluvia, and which, to use his own words, *viri odoratu valentes hic, Lipsiæ distinsse senserunt*; so that must we conclude, that our noses are not so good as those of the gentlemen at Leipzig? Mr. Winkler does not even say in his last letter, that he can electrify with the cinnamon-globe, and that the vapours sent from it are to be smelt at the entrance of the chamber; but that, with a great deal of management, they are to be perceived within an inch or two of the globe; which however we had the mortification not to be sensible of with the greatest attention.

Upon

Upon the whole, what shall we say? Shall we believe, that Mr. Winkler, relying too much upon the honesty and veracity of Mr. Pivati, and his pretended extraordinary discoveries, suffered his heated imagination to dictate his first letter to Dr. Mortimer; and that what he then sent, he rather hoped would prove true upon experiment, than what really was so? and that his second letter, in which there is so remarkable a diminution of what was promised in the first, was the retreat of one, who was unwilling to be thought to have communicated to the Royal Society any thing, which would not upon trial come out as he had represented it? But be that as it may; as success both here and abroad has been wanting to the endeavours of those, who have desired to repeat these experiments, I shall determine nothing myself; but, from an undisguised representation of the facts, as they have appeared to me, I shall leave every one to deduce his own conclusion concerning the reality of them.

XXXV. *An Account of the Bishop of London's Garden at Fulham; by Mr. William Watfon, F. R. S.*

To the Royal Society:

Gentlemen,

Read June 27.

1751.

**I** SOME time since communicated to you an account of what remained of the famous garden of John Tradescant at South Lambeth,  
H h which

which you did me the honour to receive favourably : Upon the strength of which I now lay before you the remains of that still more famous botanic garden at Fulham, wherein Dr. Henry Compton, heretofore bishop of London, planted a greater variety of curious exotic plants and trees, than had at that time been collected in any garden in England.

This excellent prelate presided over the see of London from the year 1675 to 1713; during which time, by means of a large correspondence with the principal botanists of Europe and America, he introduced into England a great number of plants, but more especially trees, which had never been seen here before, and described by no author : and in the cultivation of these, as we are informed by the late most ingenious Mr. Ray \*, he agreeably spent such part of his time, as could most conveniently be spared from his other more arduous occupations.

From this prelate's goodness in permitting with freedom persons curious in botany to visit his garden, and see therein what was to be found no-where else; and from his zeal in propagating botanical knowledge, by readily communicating to others, as well foreigners as our own countrymen, such plants and seeds, as he was in possession of, his name is mentioned with the greatest encomiums by the botanical writers of his time; to wit, by Herman, Ray, Pluknet, and others.

Mr.

\* Hist. Plant. Tom. II. p. 1798.

Mr. Ray \*, in the second volume of his history of plants, which was published in the year 1688, gives us a catalogue of the rare and exotic trees and shrubs, which he had just before observed in the bishop's garden, which he at that time called *hortus cultissimus, novisque et elegantioribus magno studio nec minore impensa undique conquestis stirpibus refertissimus*.

As this prelate's length of life and continuance in the see of London were remarkable, so we find the botanists, who wrote after Mr. Ray, most frequently mentioning in their works the new accessions of treasure to this garden; and of this you meet with a great variety of examples in the treatises of Dr. Pluknet, Herman, and Commelin.

Botanical, much more even than other worldly affairs, are subject to great fluctuations; and this arises not only from the natural decay of vegetables, and their being injured by the variety of seasons, but also from the genius and disposition of the possessors of them. So here, upon the death of bishop Compton, all the green-house plants and more tender exotic trees were, as I am informed by Sir Hans Sloane, given to the ancestor of the present Earl Tylney at Wanstead. And as the successors of this bishop in the see of London were more distinguished for their piety and learning, than for their zeal in the promotion of natural knowledge, the curiosities of this garden were not attended to, but left to the management of ignorant persons; so that many of the hardy exotic trees, however valuable, were removed, to make way for the more ordinary productions of the kitchen-garden.

H h 2

I thought



I thought therefore, that the state of this garden, after the revolutions of much more than half a century since what Mr. Ray wrote thereof, would be an acceptable present, not only to the Royal Society, but to such persons likewise, as are curious in these matters.

*A Catalogue of the exotic Trees remaining in the Bishop of London's Garden at Fulham, June 25, 1751.*

*Abies foliis solitariis, apice acuminatis.* Hort. Cliff. fort, 449.

*Abies taxi folio, fructu sursum spectante.* Tourn. 585.  
The silver fir.

*Acer platanoides.* Munting. Histor. The Norway maple.

*Acer Virginianum, folio majore subtus argenteo, supra viridi splendente.* Plukn. Phyt. Tab. 2. Fig. 4.  
The Virginian flowering maple.

*Acer maximum, foliis trifidis vel quinquefidis, Virginianum.* Plukn. Phyt. Tab. 123. Fig. 4. The ash-maple, vulgo.

*Arbutus folio serrato.* C. B. P. 460. The strawberry-tree.

*Benzoin.* Boer. Ind. alt. II. 259. The Benjamin-tree.

*Cedrus Libani.* Barrel. rar. Tab. 499. Cedar of Libanus.

*Celtis foliis ovato-lanceolatis serratis.* Hort. Cliff. 39.  
Lotus arbor. Cæfalpin.

*Cupressus ramos extra se spargens, quæ mas Plinii.*  
Tourn. 587. The male cypress.

Cupressus

*Cupressus meta* in fastigium convoluta, quæ fœmina  
Plinii. Tourn 587. The female cypress.

*Fraxinus florifera botryoïdes*. Morrif. Præl. Bot.  
265.

*Fraxinus folio rotundiore*. C. B. P. 416. The manna  
ash.

*Gleditsia*. Gron. flor. Virgin. 193.

*Acacia Americana triacanthos*, &c. Pluk. Mantiss.  
The honey-locust.

*Guaiacana*, *Pisshamin Virginianum*. Park. Hist. 918.  
The Virginian date plumb.

*Ilex oblongo serrato folio*. C. B. P. 424. The ever-  
green oak.

*Juniperus Virginiana*. Herman. Hort. Lugd. 347.  
The Virginian cedar.

*Laburnum majus*, vel *Cytisus Alpinus latifolius flore*  
racemoso pendulo. Tourn. 648.

*Larix folio deciduo conifera*. J. B. Hist. I. 265.  
The larch-tree.

*Lilac laciniato folio*. Tourn. 602. Cut leaved jas-  
mine, vulgo.

*Mespilus prunifolia Virginiana non spinosa, fructu*  
nigricante. Plukn. Phyt. Tab. 46. Fig. 2.

*Morifolia Virginienfis arbor, loti arboris instar ramosa,*  
foliis amplissimis. Pluk. Phyt. Tab. 46. Fig. 2.

*Corylus maxima, folio latissimo Virginiana*. Raii  
Hist. 1799.

*Nux juglans Virginiana nigra*. Herman. Hort. Lugd.  
Tab. 453. The black walnut-tree.

*Pavia*. Boer. Ind. alt. II. 260. The red horse-chestnut,  
vulgo.

*Pinus fativa*. C. B. P. 491. The manured or stone  
pine.

*Pinus Americana*, foliis prælongis subinde ternis, conis plurimis confertim nascentibus. The cluster-pine. Rand. Hort. Chelf. 156.

*Quercus alba Virginiana*. Lark. Theat. 1387. The white or Virginian iron oak.

*Rhus foliis pinnatis ferratis*. Hort. Cliff. 110. Virginian f. mach.

*Robinia aculeis geminatis*. Hort. Cliff. 354. Pseudo-acacia filiquis glabris. Boer. Ind. II. 39.

*Ruscus angustifolius fructu summis ramulis innascente*. Tourn. 79.

*Laurus Alexandrina fructu e summitate caulium prodeunte*. Herm. Hort. Lugd. 631.

*Siliquastrum*. Tourn. 647. *Cercis foliis cordato-oviculatis glabris*. Hort. Cliff. 156. Arbor Judæ vulgo.

*Suber latifolium perpetuo virens*. C. B. P. 424. The cork-tree.

*Terebinthus Indica* Theophrasti.

*Pistachia foliis impar-ipinnatis, foliolis ovato-lanceolatis*. Hort. Cliff. 456. The pistachia-tree.

These just now recited are the remains of that once famous garden; among which are some, that notwithstanding the present great improvements in gardening, are scarce to be found elsewhere. From the length of time they have stood, several of the trees are by much the largest of their kind I ever have seen, and are probably the largest in Europe. This account of them therefore is not merely a matter of curiosity; but we learn from it, that many of these trees, though produced naturally in climates  
and

and latitudes very different from our own, have grown to a very great magnitude with us, and have endured our rude winters, some of them, for a most a century: and that they in proper soils and situations may be propagated for advantage, as well as for beauty. For the exemplification of this I would recommend to the curious observer the black Virginian walnut-tree, the cluster-pine, the honey locust, the pseudo-acacia, the ash-maple, &c. now remaining at Fulham.

I cannot conclude this paper, without testifying in this public manner my obligations to § the present bishop of London, who has with so eminent a degree of reputation filled those high stations, to which he has been called, not only for his repeated civilities to myself, but likewise for his assurances to me, that no care shall be wanting for the preservation of the very curious particulars mention'd in this catalogue.

I have the honour to be with the most profound respect,

Gentlemen,

London, June 27,  
1751.

Your most obedient servant,

W. Watfon.

§ Dr. Thomas Sherlock,

XXXVI. *An Account of an inverted Iris, observed on the Grass in September, and another in October, 1751, by Philip Carteret Webb Esq; F. R. S.*

Read Octob 24. 1751. ON the 24 September 1751, about ten in the morning, I observed a solar iris on a grass lawn, near my house, at Busbridge in Surry. The morning was fair and clear, and the grass of the lawn was the night before almost cover'd with webs resembling those of spiders, which many persons esteem the forerunners of fair weather; and there had fallen in the night a large dew, with which the webs and the grass were thoroughly wetted.

The arch or bow appeared inverted, the point *A* being distant about 24 inches from the point of my foot; and where-ever I moved on the lawn, it seemed to move at that distance before me. The lawn on which I observed this appearance, is a hanging level, which drops about 6 feet in 100 from *A* towards *E*. It extended itself to the end of the lawn, the grass of which was short, and it was not visible on the surface of the adjoining water, or grass fields.

It was about two feet wide, and the colours were vivid and distinct.

Not having seen any thing of this kind before, nor recollecting to have read a description of an iris of this sort, I upon the spot took the dimensions of it expressed in the annexed Fig. 1. in which the distance from

*A* to *B* was = 26 feet

*A* to *C* = 21 feet

*B* to *C* = 18 feet 6 inches

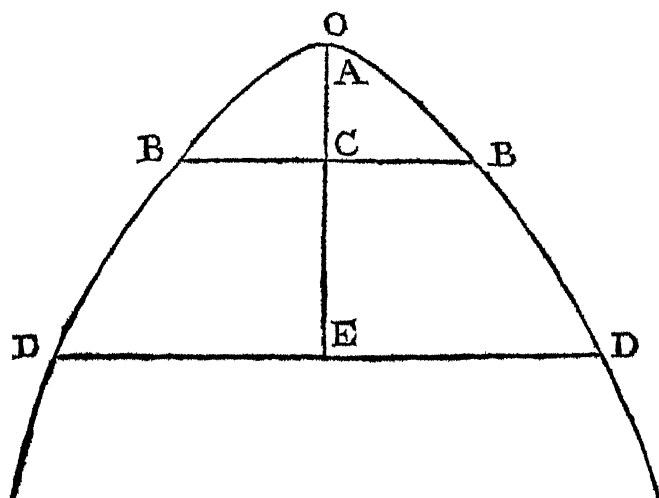
*E* to *C* = 28 feet

*E* to *D* = 40 feet

*B* to *D* = 34 feet.

The measures from *A* to *B* and *B* to *D* are something less than the truth, being the measure of a string strained in a strait line from *A* to *B*, and from *B* to *D*, and not of the curve line *ABD*.

Fig. 1.



*O*, the spectator's foot with his back to the sun, looking toward *E*, distant about 2 feet from *A*.

ON Thursday the 3 October 1751, at 30' after 9 in the forenoon, I observed about the same spot a like iris. It was a very fair morning: there had fallen a large dew in the night, and the lawn was then, and the night before, webbed over as it was the 23 September.

The dimensions of this last iris expressed in the annexed Fig. N<sup>o</sup> 2, were taken with more accuracy than the former. I measured it only to G, but it was visible much farther than the whole extent of the lawn.

In Fig. 2. the distance from

*A* to *G* was = 54 feet  $2\frac{1}{2}$  inches

*A* to *C* = 2 feet  $3\frac{1}{2}$  inches

*A* to *B* = 4 feet 6 inches

*C* to *B* = 4 feet

*D* to *H* = 16 feet

*H* to *F* = 22 feet 7 inches

*E* to *F* = 12 feet 7 inches

*H* to *K* = 37 feet

*K* to *G* = 42 feet 10 inches

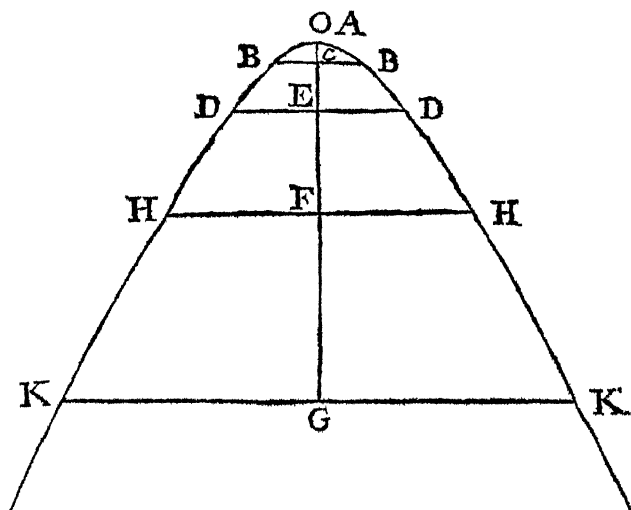
*G* to *F* = 30 feet 10 inches

*B* to *D* = 16 feet 8 inches

*D* to *E* = 12 feet 9 inches

*E* to *C* = 8 feet 6 inches

Fig. 2.



The first, if not the only author, who, I recollect, mentions this kind of iris, is Rohault, *Physica, pars* 3. *cap.* 17. *de arcu cælesti, sect.* 34. His account is in the annexed note (a).

I i 2

Remark.

(a) Neque silentio prætereunda est illa notatu dignissima observatio, quod cum hactenus aquæ guttas tanquam in aere cadentes, & per ea loca, ubi sitæ esse debent, quo colores exhibere possint, ex ordine transeuntes consideraverimus, fingi quoque potest illas in certis locis fixas esse, ubi etiamnum tantum non rotundæ sint. Utique vir eruditus matutino quodam tempore in aggre deambulans, ad alteram manum in prati latius patentis herba conspicatus est arcum, qui, prout ipse gradum proferebat, locum mutare videbatur; id quod magnam ei admirationem movit, maxime quod cælum undique ferenum esset, & nulla nube maculatum. Verum cum proprius inspectis herbis, aquæ guttas, tanquam roris stillas, prope

singulis



*Remark.*

An account of the like appearance is given by Dr. Langrish in the *Philosophical Transactions*, N° 369. The description of it agrees with this of Mr. Webb. The doctor observes, that its figure may be an hyperbola, parabola, or ellipsis, according to the angle of the intersection of the plane of the horizon with the cone of rays. That, which he saw, he took to be an hyperbola.

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singulis foliis inhærescere videret, quas e densiore nebula, quæ aerem paulo ante obscurâisset, formatas existimabat, mirari desiit; cum enim explicationem supra traditam non ignoraret, judicabat continuo istas aquæ guttas arcus cœlestis speciem exhibere, qui apparuit, quoad guttæ herbarum foliis inhæerent. Probe etiam intelligebat, eum arcum videri debere inversum, ut profecto videbatur, quod istæ aquæ guttæ inferiorem tantum superficiem coni axem aspectus ambientis partem occuparent.

XXXVII. *Extracts of several Letters from John Huxham, M.D. of Plymouth, F.R.S. and Mr. Tripe, Surgeon, at Ashburton in Devonshire, concerning a Body found in a Vault in the Church of Staverton in that County: Communicated by Thomas Stack, M. D. F. R. S.*

*Extract of a Letter from Dr. Huxham to Dr. Stack, dated, Plymouth, June 29, 1750.*

Read July 5. 1750. **I** THINK the inclosed account is very extraordinary. You may depend upon it, that it is altogether true. Mr. Tripe is a very ingenious and observing surgeon at Ashburton near Staverton. Besides, I have had it from several other persons of great probity and honour.

*Mr. Tripe to Dr. Huxham.*

S I R,

Ashburton, June 28, 1750.

**T**H E R E having been a great diversity of reports relating to a body lately discover'd in a vault in Staverton-church, I have taken the liberty of communicating to you the few following particulars; in hopes thereby to induce you to inform yourself more fully by your own inspection.

As it does not appear by the register of burials, that any person has been deposited in this vault since October 15, 1669, it is certain, that a body has lain  
there

there upwards of fourscore years: yet, when the vault was open'd about four months ago, it was found as perfect in all its parts, as if but just interr'd. The whole body was plump and full; the skin white, soft, smooth, and elastic; the hair strong, and the limbs nearly as flexible as when living.

A winding sheet, which was as firm as if but just applied, inclosed it from head to foot; and two coarse linen cloths, dipp'd in a blackish substance like pitch, infolded the winding-sheet. The body thus protected was placed in an oaken coffin, on which, as it was always cover'd with water, was found a large stone and a log of wood, probably to keep it at the bottom.

Various have been the conjectures as to the cause of its preservation; and as it has been reported, tho' probably without foundation, that the person was a Roman-catholic, there have been some of that religion, who, not having philosophy enough to account for it from natural causes, have attributed it to a supernatural one, and canonized him; and, in consequence of this, have taken away several pieces of the winding-sheet and pitch-cloths, preserving them as reliques with the greatest veneration.

In my opinion, the pitch-cloths and water overthrow the miracle, and bring it within the power of natural agents; the former, by defending the body from the external air, and the latter, by preserving the tenacity of the pitch. The left side, from the middle of the forehead to the *scrotum*, having been for some time exposed to the air, is now grown black, and moulders away; but where the pitch-cloths remain, the parts underneath are perfectly fresh and firm. As the coffin is now pretty much injured,  
tho'

tho' intirely found, when the vault was first open'd, the body is order'd by Mr. Worth, of Worth near Tiverton, whose ancestor he is, to be speedily removed to another, and then nailed up. I am

Yours, &c.

Nicholas Tripe.

*Dr. Huxham to Dr. Stack.*

Dear Sir,

Read Octob. 24.  
1751.

I AM very sorry I could not myself attend the dissection, which I had designed, but was hinder'd. Mr. Tripe however told me, he found the heart and lungs as found, as if the person had not been dead above four days, but much more flat and compressed than usual; the joints very flexible and supple; the knees in particular, the *patella*, tendons, ligaments, and the whole articulation being as smooth, unctuous, and flexible, as in a body newly dead. He also gave me a piece of the pitch-cloth, which enveloped the whole body wrapp'd up within in the linen sheet, as mention'd in the letter I formerly sent you, of which you took a copy, and to which I refer you.

I have inclosed a piece of the pitch, or tar, resin, and turpentine, with which the outer *involucrum* was besmeared. I take it to be pitch or tar, and turpentine; as it readily dissolves in *sp. vini*, and smells like it when melted.

Simon

Simon Worth Esq; whose corps this is, died at Madrid, and was sent home in the manner described, and so buried. His wife's coffin, who was buried in the same vault two years before, and two of his children about 11 years after (as appears by the register) were quite rotten. The oaken coffin, pitch-cloth, and water, seem greatly to have contributed to the preservation of this body. His coffin was found very found. I am, Sir,

Your very affectionate

May 21, 1751.

obliged humble servant,

J. Huxham.

*Mr. Tripe to Dr. Huxham.*

S I R,

Read Octob. 24.  
1751.

**A**S in a former letter I gave you some particulars relating to the external parts of the body, and its preserving apparatus, in this I shall give you an account of the internal. But before I enter upon this description, I must beg leave to observe to you, that as a great number of people resorted to the vault, on the fame of this extraordinary corps, the anterior parts of the body, from the middle of the forehead to the *pudenda*, except the right side of the *thorax*, the right *hypocondrium*, *ile*, and *inguen*, were soon stripped of the tar-cloth and winding-sheet, in order, as the different motives of curiosity or superstition prevail'd, to be preserved as  
reliques,

reliques, or to commemorate so remarkable an event.

July 9, 1749, having in the first place remov'd the body, half-cover'd with water and mud, to a convenient part of the churchyard, divested it of its coverings, and wash'd off the filth, I made an incision thro' the integuments and muscles of the left buttock, and found the *membrana adiposa* pretty near an inch thick; its *adepts* of a pale yellow, very dry, hard, and friable, and the membranous parts, except the cellular coat of the muscle, which was scarce sensibly altered, quite indistinct. The water having probably made its way thro' the *vasa absorbentia* to the *glutæus maximus*, its *fasciculi* were thin, pale, and flaccid, exhibiting the appearance of beef macerated in water; but those of the *medius* and *minimus*, especially the latter, to which it had no access, with their proper moisture and softness, still retained their natural fullness, red colour, strength, and elasticity.

In order, in the next place, to inform you of the state of the *peritonæum* and abdominal *viscera*, I made a crucial incision thro' the integuments and muscles of the *abdomen*, carrying the longitudinal section from the *cartilago ensiformis* to the *os pubis*, and the transverse from the right side of the *regio lumbaris* thro' the *umbilicus* to the left; and as the *abdomen* had been so long exposed to the air, its integuments and muscles, except the *aponeuroses* in immediate contact with the *peritonæum*, which had undergone no material alteration, were grown very black, dry, hard, and like rotten timber void of smell, and mouldering into dust. On dividing the *peritonæum*, which throughout its whole extent was of a natural

colour, found, firm, smooth, and extensible, I found the *omentum* nearly in a similar state to that of the *membrana adiposa*, full an inch thick, and extended to the lower part of the *hypogastrium*. The left lobe of the liver was of a pale brown complexion, dry, hard, and shrunk; but the right still preserved its natural dusky red colour, softness, and extension. The *vesica fellea* was quite empty, but in all other respects in a natural state; as were likewise the *ductus cysticus hepaticus* and *communis choledochus*, the *vena porta*, *ligamentum latum* or *suspensorium*, and *rotundum*. The spleen was of a pale bluish grey colour, lax incoherent texture, rough unequal surface, very dry, hard, crisp, and contracted. The stomach was somewhat inflated, and its *villi* in consequence imperceptible. There was no appearance of aliment in it, or of *feces alvine* in the intestines, but in both, as well the *intestina tenuia* as *crassa*, a blackish liquid inflammable tar-like substance, wholly soluble in oil, and for the most part in spirits of wine dephlegmated, but absolutely immiscible with water: their internal surface, especially where it was cover'd with this tar-like matter, was of a brown complexion, and somewhat rough and indurated, but their external was of a natural colour, perfectly smooth and soft. The *pancreas* was almost cover'd with a pale-colour'd dry indurated coat, and of a pale reddish complexion, but not altogether so moist and soft as in a natural state. The mesentery was wholly encompassed with a pretty large quantity of pale-colour'd fat, which in the left side of the *abdomen*, especially near the *omentum*, was quite dry and hard, and the mesentery itself in a manner indistinguishable, but in the right somewhat

what less dry and hard, and the mesenteric glands and *laminae* more distinct. The kidneys and *glandulae renales* were involved in a very plentiful portion of fat; and for the most part the ureters; that of the left kidney and *glandula renalis* resembling the fat of the mesentery in the left side of the *abdomen*, and that of the right, the fat of the mesentery in the right side: the left kidney and *glandula renalis* were nearly of the same brown complexion, but in every other respect in a state analogous to that of the spleen: the left ureter was of a natural colour, soft, smooth, and flexible; but where enveloped with fat, something hard, rough, and inflexible: the right kidney and *glandula renalis*, together with the right ureter, were in all respects in a natural state, as was likewise the *vesica urinaria*, except that it was quite empty, and its coats considerably thicken'd and contracted. The *aorta* and *vena cava*, together with their capital branches, the *receptaculum chyli* and *vesiculae seminales*, being cover'd with fat, which was likewise of a pale complexion, and more or less dry and hard, as it was situated in the right or left side of the *abdomen*, I was obliged to content myself with inspecting the large and more obvious parts; it being altogether impracticable to remove the *viscera*, as I was surrounded and press'd by near an hundred people, during the whole time of dissection.

I come now to acquaint you with the condition of the *pleurae*, and contents of the *thorax*: and in order thereto, I extended the longitudinal section of the *abdomen*, quite thro' the *parietes* of the *thorax*, on the left side of the *sternum* to the clavicle; and tho' the integuments and muscles of the *thorax* were in a



parallel state with those of the *abdomen*, the cartilages of the ribs were pretty white, firm, smooth, and elastic. The *pleuræ*, together with its duplicatures, the *mediastinum*, which I separated from the *sternum*, in order to inspect the right cavity of the *thorax*, were found, and of a natural colour, firm texture, smooth equal surface, soft and pliable. The *pericardium*, except where it adher'd to the tendinous part of the diaphragm, was invested with fat, of a pale complexion, but not altogether so dry and hard as that in the left side of the *abdomen*, and throughout inseparably conjoined with the heart, which was very large, and of a depressed figure, dry, hard, and constricted. The left lobe of the lungs near the *pericardium* was of a very pale brown colour, with a faint cast of red, considerably collapsed, somewhat dry and hard, and the investing membrane of a rough uneven surface, quite dry and rigid ; but the remaining part, together with the right, were of a redder complexion, lax spongy texture, soft and compressible, and their investing membrane of a smooth even surface, soft and flaccid. The *aspera arteria* and *oesophagus* were for the most part thinly cover'd with fat, like that of the *pericardium*, but in all respects in a natural condition, as were likewise the *aorta*, *vena cava*, pulmonary arteries and veins, and all their capital branches. The diaphragm was considerably relaxed, and of a concavo-convex figure ; and except that its muscular part was a little paler than it ought, and its tendinous, where connected to the *pericardium*, a little harder, it was in every respect according to nature. The *viscera* of the *thorax* and *abdomen* were well-proportion'd, and quite free from any  
preternatural

preternatural adhesion to one another, or to the *plurae*, diaphragm, or *peritonæum*.

I shall now describe to you the state of the parts concern'd in the articulation of the knee; and having for that purpose remov'd the integuments and muscles from the joint, I found the tendinous ligaments covering the anterior convex surface of the *patella* of a whitish complexion, firm, smooth, and flexible, and the cartilage covering the posterior, white, solid, smooth, and elastic. The burfal and crucial ligaments, the semilunar cartilages, mucilaginous glands, and the adipose substance, in which the glands were seated, were all moisten'd with *sinophia*, and in their natural order. The anterior surface of the *patella* was somewhat rough and black; but the posterior, together with the processes and cavities in the superior part of the *tibia*, and the *apophyses* and cavity in the inferior part of the *os femoris*, were in all respects in a natural state.

I shall finish this account of the dissection with a description of the state of the tendons in the left arm, near the wrist, together with that of the *occipitalis* muscle, *pericranium*, and *os occipitis*. As to the former, tho' the integuments and muscles were black and mouldering, the tendons were of a whitish colour, close contexture, hard and smooth; and as to the latter, having separated the integuments in the *occiput*, I found the *occipitalis* muscle quite red, moist soft, strong, and contractile, the *pericranium* tolerably white, firm, smooth, and unelastic; and the *os occipitis* of a very firm and solid texture, somewhat rough and black, but, on scraping off the surface, smooth, and of a natural colour.

Having

Having thus given you the particulars of the dissection, I must impose yet a little more on your patience, while I communicate to you the state of the external parts of the body describ'd in the former letter, as it appear'd at the time of dissection; as likewise that of some others, which have hitherto been unobserv'd. To begin with the first: the body was somewhat extenuated, and the skin of a dark complexion; but, except where it was expos'd to the air, firm, soft, smooth, and flexible. The hair, for the most part, was separated from the scalp; it was pretty thick, and of a blackish colour, with a few grey ones intermix'd, about five inches in length, soft, strong, and elastic: there was no appearance of any besides in any other part of the body; but I was informed by Mr. Preston, the present proprietor of the vault, and a surgeon's apprentice in the neighbourhood, who saw the body, when it was first discover'd, that it had then a dark-colour'd beard, about three inches and a quarter in length. The joints were altogether as flexible as in a natural state. The *tunica adnatá* of each eye was of a loose contexture, quite rough and discolour'd, and the *cornea* opaque, flat, and wither'd. As the head lay near the entrance into the vault, some one, in getting down, had probably stepp'd on his face, and thereby considerably depress'd all the lower part of the nose, and forced a few of the *dentes incisores* out of their sockets. The integuments and muscles, especially those of the depressed part of the nose, were quite consum'd, and the cartilages following their fate. The teeth were exceedingly hard, and firmly fix'd in their sockets, somewhat rough, and of a blackish colour. All the  
tongue

tongue was consum'd, except its investing membrane, which was likewise of a blackish colour, and wasting away. The integuments and muscles of the face, from the middle of the forehead to the chin, were become black, and crumbling into dust. The *pu-denda* were quite reduced to their membranes, which were also become black, and mouldering away. The nails were grown about the third part of an inch beyond the fingers and toes, and excepting a little alteration in colour, in every respect in a natural state.

I shall now restrain my pen from being any further tedious, and hope what I have communicated will be acceptable to you.

Ashburton, Sept. 18.  
1751.

Your most obedient servant,

Nicholas Tripe.

XXXVIII. *Extract of a Letter from Professor Euler, of Berlin, to the Rev. Mr. Caspar Wetstein, Chaplain to Her Royal Highness the Princess Dowager of Wales.*

S I R,

Read Oct. 24. 1751. **Y**OU have heard, without doubt, that that the Academy at St. Petersburg have fixed a prize of one hundred ducats, which they will give every year to him, who shall give the best answer to the question, that shall be proposed; and for the first time they have proposed this question:

“ Whether

“ Whether the theory of Sir Isaac Newton is sufficient to explain all the irregularities which  
 “ are found in the motion of the moon?

This question is of the last importance; and I must own, that, till now, I always believed, that this theory did not agree with the motion of the apogee of the moon. Mr. Clairaut was of the same opinion; but he has publicly retracted it by declaring, that the motion of the apogee is not contrary to the Newtonian theory. Upon this occasion I have renew'd my inquiries on this affair; and, after most tedious calculations, I have at length found to my satisfaction, that Mr. Clairaut was in the right, and that this theory is intirely sufficient to explain the motion of the apogee of the moon. As this inquiry is of the greatest difficulty, and as those, who hitherto pretended to have proved this nice agreement of the theory with the truth, have been much deceived, it is to Mr Clairaut that we are obliged for this important discovery, which gives quite a new lustre to the theory of the great Newton: and it is but now, that we can expect good astronomical tables of the moon.

XXXIX. *Extract of Two Letters from Dr. Alston, Bot. Prof. at Edinburgh, to Dr. Mortimer, Secr. R. S. The first dated 17 March, 1749; the second, August 9, 1750.*

Read Oct. 24,  
1751.

**A** PROPERTY of quick-lime, which I believe was not observed before. In June 1743, for some experiments in vegetation, I infused about 2 pounds of quick-lime in 24 pounds of water, resolving to change the lime, so soon as it did not communicate its virtues to the water. I soon made use of the first lime-water, and filled the vessel with fresh water. When that was exhausted, I fill'd it up a third time; and so on for twenty or thirty times: for I had no reason to change the lime for three years; so long it was good lime-water, gather'd crusts on its surface, turned syrup of violets green, vegetable infusions yellow, tasted as at the first. But at the end of the third, it gather'd no more crusts, was no more lime-water.

The quick-lime, which I kept dry, fell soon into a powder; it stood cover'd these three years (the vessel with the lime-water in it was an inverted large bell-glass, never cover'd) in the green-house. This powder I infused in water, but it communicated no virtue to it whatever. This perhaps you will difficultly believe, but it is easy to make the experiment. The *calx vive*, that I used, was made of the common lime-stone. It is also a common observation of our farmers, that the effect of lime on lands lasts only 3 years.

*Second Letter, August 9, 1750.*

THE paradox, which I formerly mention'd, concerning *calx vive*, which no body would at first believe, I have demonstrated by repeated experiments, by which it appears, that the stone *calx vive* may afford more than six hundred times its own weight of good lime-water ; for from half a drachm of quick-lime I had forty ounces of lime-water ; from one pound of quick-lime 500 pounds of lime-water ; and the lime is not yet exhausted, the water being as good now as at first, by every experiment that I know. I poured some of it cold (very lately) on some small *calculi*, in a drinking-glass, and in one night's time such phænomena appeared, as notably explained, as well as confirmed, the use of lime-water in the stone. I found also, that quick-lime kept dry, in the open air, 14 months, communicated nothing to water, tho' long infused in it : that lime-water, boiled down to a fourth part, is not weaken'd, neither sensibly stronger ; yet yields a very little of small slender prismatic crystals. I am, Sir,

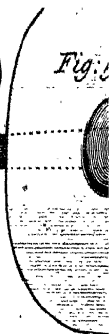
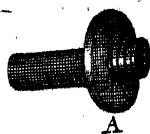
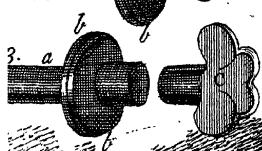
Your obliged most humble servant,

Charles Alston.











**XL.** *A new Trocart for the Puncture in the Hydrocephalus, and for other Evacuations, which are necessary to be made at different Times; by M. le Cat, F. R. S. Translated from the French by Tho. Stack, M. D. F. R. S.*

Read Oct 31.  
1751.

**O**N the 15 of October, 1744, Peter Michel, an infant of three months and a half old, son of a weaver, of the suburb of St. Sever of Rouen, was brought to me, having his head, for five weeks past only, as big as it appears in Fig. 1. All the sutures of the scull were considerably separated, asunder; the exterior veins of the head very much swoln, and the eyes turned downward. This infant was pretty plump, and had had no distemper before this accident; but from the time it appear'd, he became very froward, far from being dull or lethargic, as some authors say.

A hydrocephalus of so enormous a size, and so speedily formed, appear'd to me incurable by medicines in so young an infant; and entertaining no greater hopes from the operation, I exhorted the parents to patience. They came again to me, and earnestly intreated me, saying, that their child could not possibly hold out long against a distemper, which gain'd ground so very fast. They took the event on themselves, and by force of intreaties made me resolve on the operation.

I suspected, that the cause of the deaths (and sudden too for the most part) of those, who had been

punctured for the hydrocephalus, might probably be, that all the water had been drawn off at once; and that the brain had been left, as it were, uncover'd, and expos'd to the impressions of the air, which must necessarily fill the wide space, that had been occupied by the water; since, in this case, the integuments could not be press'd close on the contained parts, as it happens to the integuments of the *abdomen* after the puncture in the *ascites*. Wherefore, since I was prevail'd on to make the puncture, I resolv'd to draw the water by little and little, at different times distant from each other; and in the intervals of these evacuations to compress the integuments with a proper bandage, to make them come near the brain.

The common trocars did not seem proper to fulfil these views. I was of opinion, that punctures often repeated in these nervous parts were dangerous: besides, as the integuments of the head were thin, and upon the stretch, the opening being once made would never close sufficiently to stop the evacuation, when the *canula* was removed; and if I left the *canula* in the orifice, and stopp'd it with a stopple, this same disposition of the integuments would suffer the water to ouze out between them and the sides of the *canula*: thus would the evacuation become total, in spite of me, whatever method I us'd with the trocars already known. These reflections made me contrive the following instrument.

It is a new trocar, represented by Fig. 2. and which has this peculiarity, that the *canula* is much shorter than ordinary. This *canula* is represented separate in Fig. 3.: but there ought to be several,  
of

of different lengths for different cases. On the upper part of this *canula* there are two circles, each one of which is fasten'd to a different piece. These pieces are exhibited separate in Fig. 4. and they are made so as to be screw'd one on the other. These circles are somewhat concave in their surfaces, which correspond reciprocally; so that their circumferences touch, while there is a tolerable vacuity towards their centre. By means of this simple mechanism, I apply the plaster *x*, with a hole in it, on the lower circle *A*, whose screw passes into the hole of the plaster: this done, I screw the upper piece *B* on the lower *A*, and I squeeze the plaster tight between these two circles. The instrument becomes then as in Fig. 5. The plaster, which I have chosen, is that of Andreas a Cruce; but one may use Burgundy-pitch, or any other powerful emplastic, at pleasure. My plaster was three inches broad. To the upper end of the *canula* I adapted a very exact silver stopple *c*, Fig. 3. The part, where I intended to make the puncture, was shaved, wider than the plaster.

Thus having prepared every thing, and the *canula* being armed with its trocart, and fortified with the plaster, as it appears Fig. 5. I performed the puncture on Friday the 23 of October 1744, by thrusting in the trocart and *canula* up to the circles and plaster, which I applied and made to stick in all its parts on the head, by pressing it with my hand and fingers made very warm, and also with hot linen-cloths. When the plaster was thoroughly well fasten'd on, I pull'd out the trocart, and drew four or five ounces of serosity, of a brownish white, or  
the

the colour of pale white-wine, and somewhat foul: after which I cloied the *canula* with its stopple *c*.

By chemical experiments, this liquor was found to be neither acid nor alkaline: being put on the fire, it evaporated quite away, and left at bottom a frothy neutro-saline sediment.

Saturday, Oct. 24, I unstopp'd the *canula*, and drew the same quantity of water. The infant was ill on the Sunday: wherefore I did not disturb him that day. Monday the 26 he was better. I drew five more ounces of water. Tuesday I suffer'd him to take rest. Every time that I made this evacuation, I bound the head with a strong capeline \*. Notwithstanding these precautions, the infant died in the night between Tuesday and Wednesday; and it will presently appear, that this hydrocephalus was of an incurable sort. I open'd it, and found the brain applied against the *dura mater* as usual; but this brain was thin, and as it were spread out: it only formed a kind of thin sack fill'd with water. I open'd, and saw that the disease was nothing more than an excessive dilatation of the two lateral ventricles, by the waters collected therein. The *glandula pinealis* was almost wasted, as well as the *plexus choroides*, of which some few vestiges only remain'd. On the contrary, the other vessels, which lined the inside of this sack, were very visible.

As the brain is a soft *viscus* without elasticity, it manifestly appears, that it could not possibly resume its natural form, how slowly soever I had evacuated the  
the

\* A bandage peculiar to the head.

the waters: but perhaps the operation would have succeeded, if the seat of the dropfy had been on the outfide of the brain. However that be, this trocart to me seems uicful for feveral operations: and this is my firft motive for prefenting it to the Royal Society. My fecond motive for fo doing is, the confequences, which may be deduced from this obfervation with regard to the apoplexy.

How can one believe, that the apoplexy is caufed by the extravafation of the liquids, or by the fullnefs of the veffels, after having feen a brain filled with water, and diftended fo vaffly as this was, without any one apoplectic fymptom? Verduc, who in his pathology propofes an objection fimilar to this againft his own fystem, endeavours to folve it, but has not fucceeded. The objection remains victorious.

Nevertheless, when the brain of a perfon dead of an apoplexy is open'd, and extravafated blood is found in it, his death is imputed to this extravafation alone, and the apoplexy is pronounced fanguineous. This has happened on the death of M. De Frequienne, prefident of our parliament. On opening him I found about a tea-fpoon full of blood extravafated within the *medulla oblongata*, between the third and fourth ventricle, at the beginning of the latter. Could fo fmall a quantity of blood prefs on the principles of the nerves fo as totally to intercept the courfe of the fpirits? No, certainly; for this would be miftaking the effect for the caufe. This extravafated blood was but an accident owing to the convulfive motions of the *dura mater*, and of the veffels of the whole bafis of the fcull, feized with the apoplectic diforder, which moft commonly is  
nothing



nothing else but the matter of the gout or rheumatism fixing on this source of the nerves. Now this general attack, which swells and distends the *aura mater* throughout this whole basis, makes the blood stagnate in the vessels, some of the weakest of which burst, and at the same time closes all the canals of the nerves, and consequently kills the patient. Unless a person would chuse to say, that those broken canals were those, which concurred in the substance of the brain to the formation of the spirits, that give motion to the heart: which opinion is not free from difficulties; since it is well known, that this organ receives the influences of several nerves at a time, all which ought to bear their part in this accident, which, after all, is but the rupture of a simple capillary vessel.

The drift of these reflections is to engage practitioners to have somewhat less confidence in their theories, and, for example, not to make a poor apoplectic patient die under the lancet; a thing, which I have seen several times, from the notion which they hold, that it is the over-great quantity of blood, that kills: for, besides that this false opinion is fatal to this patient in particular, it will still be so to all future apoplectics, if the prejudice in favour of this theory be such as to prevent seeking the true causes, and the real remedies of the apoplexy.

XLI. *Observations on the Effects of the Vitrum Antimonii ceratum, by Monsr. Geoffroy, of the Royal Academy of Sciences, and F. R. S. Translated from the French by Tho. Stack, M. D. F. R. S.*

Read Oct. 31. 1751. **T**HIS medicine, the preparation of which was first published in the *Edinburgh Medical Essays*, is made by mixing an ounce of the glass of antimony in powder with a drachm of yellow wax. This mixture is kept in an iron ladle over a slow clear charcoal-fire about half an hour, taking care to stir it continually with an iron spatula, until the wax is consumed, and ceases to emit fumes. Such is the process of the preparation, published in the *Edinburgh Essays*.

In the memoirs of the Royal Academy of Sciences for the year 1745, I gave the detail of this operation, with some remarks on the changes, which wax may occasion in the glass of antimony.

Of all the preparations of glass of antimony this is doubtless the most perfect; for it is infinitely superior to the *chylista* of Hartman. This *chylista* is nothing more than a glass of antimony well pounded, and opened by acids, and then digested in spirit of wine impregnated with mastic; which never can cover the particles of this glass with coats of equal impenetrability with those form'd by wax bituminized by burning.

This medicine succeeds equally in bloody-fluxes, diarrhoea's, simple loosenesses, quartan agues, even

the most obstinate, and in certain cases of the *fluor albus*.

It must be given with caution, beginning with a very small dose, as one, two, or three grains, especially when it has been levigated again after its calcination : and thus it may be safely given to children, and even to pregnant women.

In giving it to robust persons, I always began by a small dose, as 4 or 5 grains, which I gradually increased to 18, according to the effects produced by less considerable doses. This medicine, which sometimes vomits or purges, sometimes also cures, especially in robust constitutions, without producing any visible effect.

By gradually increasing the dose of this medicine, I have given as far as 24 grains at a time, which had no other effect, but to procure two or three moderate stools the next day : but in this case it would be imprudent to continue its use without interruption ; because, as it passes slowly, the dose may possibly unite with the first at the time, that it begins to operate ; and these two doses thus joined might cause a superpurgation, which is always to be dreaded.

I should never have ventured to give this medicine to pregnant women, if chance had not convinced me, that it is not more dangerous for them than for others, when given with caution. For, among several women, whom I cured of bloody-fluxes with this medicine, there were some, that were actually with child, and did not know it themselves, at the time of their taking it. They were all cured, and no accident happen'd to any of them.

In

In pursuance of this observation, I thought I might try it, with all imaginable precautions, even on sucking children. In the mean time I was very attentive to the effect of the medicine. When the first dose vomited or purged sufficiently, I did not increase the second. Sometimes I diminished it, or even totally laid it aside for some days.

When this medicine produces nothing more than keckings at stomach, and a plentiful expectoration of thick slime, the dose may be safely increased half a grain or a grain every day. And this slight augmentation of the dose does not hinder the effect of the medicine from diminishing, in proportion as the patient comes nearer a perfect cure.

When the patient has been purged too violently by one of the first doses of this medicine, which are always small, it is a proof of the weakness of the patient; and then I give it to him but every second or third day. The distance of time observed between the doses of this medicine makes it operate less briskly, and more equally.

When the *vitrum antimonii ceratum* vomits, the patient is to drink warm water at every motion.

When the dysenteric flux is attended with sharp pains in the *abdomen*, with heat and tension, the *vitrum antimonii* is not to be given, till the pains are removed by emollient clysters, and other proper remedies.

I have not observed any difference in the effects of this medicine, whether the patient had, or had not, been bled or purged; whether the disease were recent, or of long standing; whether in fine it were attended with a fever, or not. They were all cured

equally well ; agreeable to what is said in the Edinburgh observations.

The *vitrum antimonii ceratum* is a good febrifuge. Three or four days use of this medicine generally suffices for removing the fever accompanying diarrhœa's, loosenesses, &c. But, in order to its having this effect, it must either purge or vomit the patient ; otherwise it cures the looseness, but the fever continues, and requires a very long use of the medicine to cure it. When it operates in a sensible manner, it generally gives the patient an appetite, when he is near being cured : but the weakness of his stomach does not allow his giving way to it, without running great risks.

When this remedy operates a cure without producing any visible effects, it would be dangerous to increase the dose till it causes evacuations : for, unless the patient be of a strong constitution, you endanger the bringing on a hypercatharsis.

Moreover I have observed, that the fineness of the powder has a great influence on the manner of its operation. That, which is very fine, is much more active, than that which is somewhat less so : for example, a grain of the *vitrum antimonii ceratum* reduced to a very fine powder will have more force and action, than two grains of the same glass reduced to a powder somewhat less fine. Wherefore I always prefer'd the first sort, as productive of more certain effects, and less incommoding the stomach.

The vegetable acids develope and increase the emetic quality of this medicine to such a degree, that you would always put the patient's life, who takes it, in great danger, if you did not absolutely forbid

forbid him the use of acid fruits, and aliments, that are liable to turn sour, as milk, wine, &c.

This medicine succeeds equally well in terine evacuations. In these cases it must be continued 15 or 20 days, giving it every other day, according to the patient's strength, or the quantity given at a dose.

With this medicine alone I have likewise cured a girl of eighteen, who had the *fluor albus* abundantly from the age of twelve. At first I gave her three doses for three days together. The first dose was half a grain, the second a grain, and the third a grain and half. The two first made her vomit very gently, but the third purged her plentifully. After some days of rest I repeated the same three doses. During this time the discharge was much greater than usual, and it changed colour several times. At the end of eight days the patient had her courses in larger quantities than ordinary. Some days after her courses were over, the *fluor albus* appeared again, but was much diminish'd; and by continuing to give the same doses of this medicine every week for two months, the patient was perfectly cured.

In obstinate quartan intermittents, which had resisted the most powerful febrifuges, I have given this medicine on the two days of intermission, omitting it the day of the paroxysm; and continuing it thus, and increasing the dose very gradually, the paroxysms grew considerably weaker; and generally the fourth did not return. The patients, whom I cured in this manner near a year ago, have never had the least return of the fever.

Excepting in the cases of fevers, all the patients, who used the *vitrum antimonii ceratum*, drank habitually of a ptisan made with rice, oatmeal, or  
hartshorn.

hartshorn. These ptisans prevent the pains of the stomach, which this medicine sometimes occasions.

I have always given this medicine in a bolus incorporated with the bitter extracts, or cordial electuaries; by which method we partly guard against the pains of the stomach. Great care ought to be taken, not to make it up with conserves or syrups of acid fruits, for the reasons already given.

I am in hopes, that, notwithstanding the prejudice, which prevails against this preparation, it will be used with success in all the cases above-mention'd; provided attention be given to the observations, which I have made in this paper. And it is to Dr. Pringle that we are indebted for an excellent medicine, which may be brought into familiar use, if people accustom themselves to administer it with prudence.

XLII. *Extract of a Letter from John Brown-  
ing Esq; of Barton-Hill near Bristol, to  
Mr. Henry Baker, F. R. S. concerning a  
Dwarf.*

Dear Sir, Barton-Hill, Sept. 12, 1751.

Read Nov. 7, 1751. **I** AM just returned from Bristol, where I have seen an extraordinary young man, whose case is very surprising. He is shewn publicly for money, and therefore I send you the printed bill, which is given about to bring company; and also a true copy of a certificate from the minister of the parish, where he was baptized, together with the attestation of several of the neighbours of great credit and

and veracity, some of whom are personally known to me. To these I have likewise added my own observations, as necessary to clear up the case.

The certificate is as follows :

“ THIS is to certify, that Lewis Hopkin, the bearer  
 “ hereof, is a man of a very honest character, and  
 “ has six children. His second son Hopkin, whom  
 “ you see now with him, is in the fifteenth year  
 “ of his age, not exceeding two feet seven inches  
 “ in height, and about 12 or 13 pounds weight,  
 “ wonderful in the sight of all beholders.  
 “ The said little man was baptized the 29 of  
 “ January 1736, by me

R. Harris,

Vicar of Llantriffert, Glamorganshire.

We have seen the above-mention'd youth, and have reason to think the contents above-mention'd to be true as set forth.

Edmund Thomas  
 Cha. Edwin  
 Matt. Deen  
 Hopkin Rees  
 Anthony Powell  
 David Thomas  
 Nich. Price  
 Wm. Cadogan.

The gentlemen, that have seen the youth, and have signed their names, are all of figure and fortune in the county of Glamorgan. Mr. David Thomas  
 lives.



lives in London, is an entry-clerk in the court of Chancery, and supplies country attornies with their writs, and lives in a lane or court near Gray's-Inn, Holborn.

I went myself to view and examine this very extraordinary and suprising, but melancholy subject; a lad entering the fifteenth year of his age, whose stature is no more than two feet seven inches, and weight thirteen pounds; labouring under all the miseries and calamities of very old age; being weak and emaciated, his eyes dim, his hearing very bad, his countenance fallen, his voice very low and hollow; a dry husky inward cough, low and hollow; his head hanging down before, so that his chin touches his breast; consequently his shoulders are raised, and his back rounded, not unlike a hump-back. His teeth are all decay'd and rotten, except one foretooth below. He is so weak, that he cannot stand erect without a support.

The father and mother both told me, that he was naturally sprightly, tho' weakly, until he was seven years old, would attempt to sing and play about, and then weighed nineteen pounds, and was as tall as, if not taller than, at present, naturally strait, well-grown, and in due proportion: but from that period he hath gradually declined, and grew weaker, losing his teeth by degrees, and is now reduced to the unhappy state I have been describing. The mother is a very jolly healthy woman, in the prime of life: the father enjoys the same blessing. They both as-  
sure

sure me, this lad has a sister about ten years of age in the same declining state. I am,

Dear Sir,

Yours most affectionately,

John Browning.

As new-born children frequently exceed in weight this youth of fifteen years, I take the liberty to communicate his case, believing it will not be thought incurious.

H. Baker.

XLIII. *A Letter from Mr. Rich. Dunthorne to the Rev. Dr. Long, F. R. S. Master of Pembroke-Hall in Cambridge, and Lowndes's Professor of Astronomy and Geometry in that University, concerning Comets.*

S I R,

Cambridge Oct. 5, 1751.

Read Nov. 14,  
1751.

**T**HERE is a manuscript in your college library, chiefly astiological, wherein there are five tracts of different authors concerning comets. One of them, intituled, *Traētatus fratris Egidii de cometis* (written on account of a comet, which appeared in the year of our Lord 1264) contains these passages relating to its place and motion:

N n

Prolog.

Prolog. “ Stella caudata seu crinita apparuit in  
 “ regno Franciæ in oriente ante solis ortum a 19° ka-  
 “ lendas Augusti usque 5° nonas Octobris in anno  
 “ Domini 1264.

Cap. 1. “ Cometem, cujus occasione hæc scripsi-  
 “ mus, primo vidimus extra circulum zodiaci versus  
 “ aquilonem contra cancrum, et demum eundem  
 “ vidimus extra circulum versus austrum sub geminis  
 “ inter canem et orionem.

Cap. 3. “ Vidimus autem et stellam caudatam,  
 “ cujus occasione hoc scripsimus, præter motum cir-  
 “ cularem diurnum, æque moveri motu retrograda-  
 “ tionis, et nulli alii similis, secundum latitudinem  
 “ ejus, quæ est a septentrione ad austrum. Visus est  
 “ moveri per duos menses solares plusquam 40 gra-  
 “ dus, vix per 3 gradus longitudinis permutans situm.

Cap. 7. “ Cometes, cujus occasione hæc scripsi-  
 “ mus, primo visa est in vespere post solis occasum,  
 “ demum post paucos dies solem pertransiens in  
 “ mane circa octavum gradum cancri, et ex hinc  
 “ cito processit retro in geminos: ——— vidimus  
 “ autem et cometem moveri ab aquilone ad austrum,  
 “ secundum latitudinem quidem plus 50 graduum, et  
 “ secundum longitudinem quidem vix 5 gradus proces-  
 “ sisse.”

Hevelius in his *Cometographia* has also given us  
 the following paragraph, among others, concerning  
 this comet:

“ A. C. 1264, stella, quæ dicitur cometes, appa-  
 “ ruit, videlicet in oriente, ante ortum diei, post stel-  
 “ lam matutinam: apparuit, scilicet, ante auroram  
 “ cum radiis multis: ipsi ejus radii longe lateque  
 “ apparuerunt

“ apparuerunt antequam oriretur ipsa stella cometes.  
 “ Igitur veloci cursu laboravit ipsa stella cometes, ita  
 “ quod præcurrerit & longe versus meridiem præcessit  
 “ stellam matutinam, i. e. luciferum. Visa est circa  
 “ festum S. Mariæ Magdalenzæ, & usque ad octavam  
 “ S. Augustini apparuit. *Compilat. Chronol.*”

Although this whole account be very slender and rude, it is however much the best I have met withal of any comet earlier than that, which was observed by Regiomontanus in the year 1472 (except perhaps the account given us by Nicephorus Gregoras of the comet of the year 1337, whose orbit is computed by Dr. Halley): for which reason, I was induced to try, whether I could investigate a set of elements capable of representing the places of this comet agreeable to the above description, and after several attempts, some of them indeed but tentative, I fixed upon the following numbers for that purpose, *viz.* the place of its ascending node in  $\text{♊}$   $19^{\circ}$ , the inclination of its orbit to the plane of the ecliptic  $36^{\circ}\frac{1}{2}$ , the place of its perihelion in  $\text{♋}$   $21^{\circ}$ , its perihelion distance from the sun 44500 such parts as the mean distance of the earth from the sun contains 100000, and the time of its being in perihelion July 6<sup>d</sup> 8<sup>h</sup> *p. m.* The motion of the comet in this orbit was direct.

Its places computed from these elements are as in the following table.

Time.	Comet's long.	Comet's latit.
July 1 in the evening	25 57	22 13 North
† 14 in the morning	7 58	10 49
18	4 8	5 2
22	1 40	0 30 South
26	0 16	5 31
30	29 35	9 59
Aug. 3	29 21	13 58
7	29 23	17 30
11	29 34	20 42
15	29 49	23 39
19	0 22	26 23
23	0 12	28 58
27	0 17	31 24
31	0 16	33 42
Sept. 4	0 13	35 56
8	29 36	38 4
12	29 0	40 6
16	28 12	42 4
20	27 7	43 55
24	25 46	45 41
28	24 8	47 18
Oct. 3	21 44	49 8

Here it might be seen in the evening after sunset \*.

Here, in the southern parts of Europe, it arose *ante ortum diei post stellam matutinam*.

Here it was *inter canem et orionem*.

\* Perhaps the tail might not be conspicuous enough to occasion its being taken much notice of, in its descent towards the perihelion.

† July the 6, the comet was in the same right ascension with the sun, and had near  $41^{\circ}\frac{1}{2}$  north declination; so that in the south of France it set about the going down of twilight, and did not rise again till day-break; and therefore might escape being seen for a few days, either morning or evening, about this time.

I think

I think the computed place here set down agree as well with the foregoing description as any regular *computus* can be expected to do, and the resemblance of all the elements gives some ground for conjecture, that this comet might possibly be the same with that which was observ'd by Paul Fabritius and others in the year 1556, whose orbit Dr. Halley has computed: See his *Synopsis Astronomiæ cometiciæ*. Indeed the change in the place of the perihelion may perhaps be thought greater than could arise from the mutual gravitations of the comets disturbing one another; but then it may be consider'd, that neither the place nor time of the perihelion, nor the perihelion distance of the comet of the year 1556, could be determined very accurately from observations made only for 12 days, at 40 days distance from the perihelion, as those of Fabritius were, unless they had been more exact than his appear to be. If these were one and the same comet, its period is 292 years; and we may expect its return about the year 1848.

There are in the before-mention'd manuscript, besides the passages already quoted from Egidius, two other places which deserve to be taken notice of. One of them is so much of a small tract, intituled, *Judicium de stellâ comata anno Domini 1301*, as concerns the place and motion of the comet; it is as follows:

“ A. D. MCCC primo, primo die Septembris appa-  
 “ ruit cometa in occidente, et per mensem vel am-  
 “ plius visus fuit. — Ultima autem die Septembris  
 “ duabus horis 40 minutis post occasum solis — in-  
 “ veni quod longitudo cometæ in signis et gradibus  
 “ erat

“ erat 20 gradus scorpionis, et latitudo \* 26 gradus  
 “ septentrionalis: Mars autem tunc erat in 20 gradu  
 “ scorpionis directus exeuns, et sic terre conjuncti  
 “ erant Mars et cometa accipiendo loca ipsorum per  
 “ circulum transeuntem per polos zodiaci. — Verum  
 “ et sexta die Octobris, scilicet in festo Sanctæ Fidis  
 “ post occasum solis eadem hora inveni quod longi-  
 “ tudo ejus erat primus gradus sagittarii, et latitudo  
 “ ejus 10 gradus septentrionalis. — Cometæ latitudo  
 “ ecliptica circa principium apparitionis suæ fuit 20  
 “ gradus et amplius septentrionalis. — Apparebat co-  
 “ meta moveri a septentrione in meridiem per oriens,  
 “ ita quod ejus longitudo orientalis continue videba-  
 “ tur augeri, et ejus latitudo septentrionalis continue  
 “ videbatur diminui. — In principio apparitionis suæ  
 “ coma protendebatur ad septentrionem; et post mo-  
 “ tum successive movebatur per orientem ad meridiem  
 “ versus stellam quæ dicitur *altayr* hoc est vultur  
 “ volans.”

Though this account is too imperfect for us to at-  
 tempt determining the orbit therefrom, it may not-  
 withstanding help us to know the same comet again,  
 if any should hereafter appear whose orbit will agree  
 with this relation; which I believe none of those al-  
 ready computed will do.

The other place I hinted at as worthy of notice,  
 is this short passage in a treatise *De significacione co-  
 metarum*:

“ Et

\* This figure (2) is a different writing from the rest of the ma-  
 nuscript, and has manifestly been alter'd since it was first written; it  
 seems to have been 10 at the first, which I think the truer reading.

“ Et nos invenimus modo quod apparuit in tempore  
 “ nostro unus cometa in principio piscium, et cauda  
 “ attingit usque ad principium geminorum in nocte  
 “ Mercurii, et hoc fuit in ultimam nocte Junii, anno  
 “ 499 Arab. et sequebatur ordinem signorum quo-  
 “ usque venit usque ad principium cancri, et dimisit  
 “ ordinem signorum, et incepit deficere.”

The word Junii here found seems to have been transcribed by mistake for the Arabic month Jumedi.j, the last day whereof that year was Wednesday Feb. 7. A. C. 1106; whereas the last day of June fell upon Saturday. This reading agrees with the following notes concerning the same comet collected by Hevelius in his *Cometographia*, p. 821.

“ A. C. 1106 a prima hebdomada quadragesimæ  
 “ cometam immensi fulgoris usque ad passionem Do-  
 “ mini conspeximus.” *Lavath ex Ursburg.*

“ A. C. 1106, mense Februar. biduo post novi-  
 “ lunium, visus est magnus cometa, ad occasum so-  
 “ lis brumalem.” *Calvis. ex Tyr.*

The new moon was Feb. 5, Ash-Wednesday that year Feb. 7, and Good-Friday, March 23.

If we suppose (with Dr. Halley) this comet to be the same with that which appeared in 1680, and that it was *in perihelio* Feb. 4, at noon (for it must have been seen in two or three days after it had passed its perihelion) some of its places would have been these;

Feb.



[ 288 ]

	d	h		Com	Long;		Corr	Lat,
Feb.	7	6	:	8	7	50	.	5 14 North
Mar.	14	7 $\frac{1}{2}$	.	8	11	49		
	19	8	.	8	15	38		
	24	8	.	8	19	2		

The wide disagreement there is between the manuscript account of this comet, and its places here computed, must very much lessen, if it does not quite overbalance, the force of the arguments brought by Dr. Halley to prove the identity of these two comets.

Indeed if this comet had been the same with that of 1680, it could not have come to the beginning of Cancer, without a change in the place of the perihelion too great to be easily admitted; nor could it have left the order of the signs without a change in the elements still greater. I am,

S I R,

Your obliged, and

most obedient servant,

Richard Dunthorne.

XLIV. *A Letter from Mr. Franklin to Mr. Peter Collinson, F. R. S. concerning the Effects of Lightning.*

S I R,

Philadelphia, June 20, 1751.

Read Nov. 14, 1751. **I**N Captain Waddel's account \* of the effects of lightning on his ship, I could not but take notice of the large *comazants* (as he calls them) that settled on the sp niks at the top-mast-heads, and burnt like very large torches before the stroke.

According to my opinion, the electrical fire was then drawing off, as by points, from the cloud; the largeness of the flame betokening the great quantity of electricity in the clouds. And had there been a good wire-communication from the spintle heads to the sea, that could have conducted more freely than tarred ropes, or masts of turpentine-wood, I imagine, there would either have been no stroke, or, if a stroke, the wire would have conducted it all into the sea without damage to the ship.

His compass lost the virtue of the loadstone, or the poles reversed, the north point turning to the south. By electricity we have here frequently given polarity to needles, and reversed it at pleasure. Mr. Wilson tried it with too small a force. A shock from four large glass jars, sent thro' a fine sewing needle, gives it polarity; and it will traverse when laid on water.

O o

If

---

\* Phil. Transf. N. 492, p. 111.

If the needle, when struck, lies east and west, the end enter'd by the electric blast points north.

If it lies north and south, the end that lay towards the north, will continue to point north. when placed on water, whether the fire enter'd at that end, or the contrary end.

The polarity is given strongest, when the needle is struck lying north and south; and weakest, when lying east and west.

Perhaps if the force was still greater, the south end, enter'd by the fire, when the needle lies north and south, might become the north; otherwise it puzzles us to account for the inverting of compasses by lightning; since their needles must always be found in that situation, and by our little experiment, whether the blast enter'd the north, and went out at the south end of the needle, or the contrary, the end, that lay to the north, still should continue to point north. I have not yet had time to read and consider Dr. Knight's *Essays*, just now received from you, which possibly may explain this.

In these experiments the ends of the needles are sometimes finely blued, like a watch-spring, by the electric flame. This colour given by the flash from two jars only, will wipe off; but four will fix it, and frequently melt the needles. I send you some, that have had their heads and points melted off by our mimic lightning, and a pin, that had its point melted off, and some part of its head and neck run.

Sometimes the surface on the body of the needles is also run, and appears blister'd, when examined by a magnifying glass. The jars I make use of hold 7 or 8 gallons, and are coated and lined with tin-foil.

Each

Each of them takes 1000 turns of a globe 9 inches diameter to charge it. I send you two specimens of tin-foil melted between glass, by the force of two jars only.

I have not heard, that any of your European electricians have been able to fire gunpowder by the electric flame. We do it here in this manner :

A small cartridge is fill'd with dry powder, hard rammed, so as to bruise some of the grains. Two pointed wires are then thrust in, one at each end, the points approaching each other in the middle of the cartridge, till within the distance of half an inch : then the cartridge being placed in the circle, when the four jars are discharged, the electric flame leaping from the point of one wire to the point of the other, within the cartridge among the powder, fires it, and the explosion of the powder is at the same instant with the crack of the discharge. I am,

S I R,

Your humble servant,

Benjamin Franklin.

XLV. *Observations on fungous Excrefcences of the Bladder; a cutting Forceps for extirpating thefe Excrefcences; and Canula's for treating thefe Difcafes; by M. Le Cat, F. R. S. Translated from the French by Tho. Stacke, M. D. F. R. S.*

Read Nov 14, 1751. **T**HE widow Néel, a farmer at Pleinboſc, in the pariſh of Etoutteville near Yvetot, had, for ſome years, felt pain in the ſmall of the back, thighs, &c. In the year 1734, ſhe had made bloody urine, and had one thigh and leg œdematous. Theſe accidents having diſappear'd, were ſucceeded by worſe ſymptoms. She had frequent calls to make water, and did it often, a little at a time, and with pain, which was violent, particularly after the urine was diſcharged: and this was of a dull red colour, that is, a little tinged with blood.

All thoſe of the profeſſion, whom the patient conſulted, affirmed her that ſhe had the ſtone; and I was of the ſame opinion, but would not pronounce poſitively, till I had ſearch'd her; which I did the 17 of October 1735. As ſoon as the ſound was introduced, blood came away, and in greater quantity, the more I moved it about. The free play of the ſound was obſtructed: I found no ſtone, but pretty ſure ſigns of excreſcences in the obſtruction of the ſound, and the iſſue of blood, which its motion occaſioned. However, by dint of management I found a ſituation of the ſound, in which, by giving a little  
jerk,

jerk, I touch'd a hard body, the dull percussion of which convey'd nothing but obscurity to my hand or judgment. In order to come at the knowledge of this body, I pass'd the crooked sound destined for men, the bent of which I thought fitter to favour my inquiries. I found the same body again, but still with the same obscurity. I had extract'd stones, which did not afford plainer marks of their existence; wherefore I judg'd, that there might be a stone and fungous excrescences too in this bladder; and that these excrescences were the obstacles that render'd our search difficult, and the stone doubtful. But the dull resistance which this hard body made, inclin'd me to think, that it might as well be some scirrhus tumour. These doubts held us a long time in suspense what party to take: but the extreme pain which the patient suffer'd, and the frequent hæmorrhages, which must soon put an end to her life, made us determine to perform the operation; that is, to open the neck of the bladder, either to extract the stone, if any, or remove and treat the fungus's, which exist'd beyond all doubt.

I cut this widow the 18 of October 1735, by what I call the *rural apparatus*, that is, without placing her upon the table used in our hospitals, which could not well be carried to the country where this woman dwelt.

I plac'd her on the edge of her bed: a chair turn'd upside down supported her shoulders. Unknown to the patient I caus'd a board to be put under the first mattrais of this edge of the bed: and when she was plac'd on it, under her backside, or the *os sacrum*, I laid another board, on which I put a straw cushion  
made

made compact and cover'd with linen-cloth. Two straps tied to the ends of this board were pass'd into the bars of the turn'd-up chair, which supported the patient's body: and these pieces, to wit, the chair and the board with the cushion were fastened together by buckles that were on the straps. The assistants, who were on each side of the patient, had each a strong large swathing band folded double, and pass'd into this fold in a slip-knot: at present I use one of those strong woollen sashes or girdles, with which couriers bind or swathe their body. This slip-knot was pass'd on the patient's wrists, who had seen nothing of these preparations, and she was bound fast, almost before she was aware of it. Then I introduced a common grooved staff, such as is used for abscesses of the bladder: I turn'd the groove towards the patient's left thigh, and on this groove I push'd my knife into the bladder; which knife is the same that I still use for women, but made a little narrower. On that knife, which had a groove, I slid the gorget and forceps in the usual manner.

I searched for the stone, but in vain, I found nothing but excrescences, one of which was considerably hard: I extracted several clusters of them with the forceps. Yet still I was not very certain, but that there might be a stone behind a rampart of excrescences which I felt; and I had not brought the crooked forceps with me to search behind this intrenchment. When I judg'd that the patient was fatigued by my searchings, and the extirpations which I made with the forceps; I had her put to bed, after having put a canula into the wound,

contrary

contrary to my usual custom ; but this case required it : these strange bodies were to be removed, if possible ; that organ must be injected and consequently the canula was absolutely necessary. The patient, who bore the operation exceedingly well, was blooded two hours after it : she had a pretty good night, and was blooded again the next morning. I left one of my pupils with her, and ret rn'd to Rouen

The canula, which I left in the wound, was of the common sort, and therefore too narrow to admit of searching in the diseased part, and to give issue to those excrescences, which we ought to endeavour to disengage and bring away in this treatment : besides, it is extremely difficult to make the canula remain in the wound.

As soon as I got to Rouen, I order'd the canula (Plate IV. Fig. 1.) to be made ; the advantages of which above the old one are :

1. To afford a wider passage for the substances that are to be evacuated and introduced.

2. To secure the instrument in the bladder, by its own structure chiefly, and particularly by the swelling at *BB*.

3. The neck *AA*, which is at the basis of the swelling, is embraced by the neck of the bladder ; whereby the surgeon may be sure, how much of the canula enters the bladder : and the openings *CC*, being immediately above the swelling *B*, are fixed at the lowest part of the bladder.

Fig. 2, 3, 4, of the same plate represent the same canula as above described, but with further improvements for cases, which require the evacuation of gross substances, the passage for which cannot  
be



be too wide and direct. The description of its parts, which is in the explanation of the figures, suffices to shew its use.

I return'd to the patient the next day; and found her in a fever, with many colicky pains: but at the end of the third day there was nothing extraordinary.

I intended to make another search, but I feared renewing those accidents: wherefore I contented myself with injecting a liquid digestive; and deferr'd any farther trials till after the suppuration was well formed, which I expected about the 8th or 9th day.

I re-visited the patient on the 7th, and found her a little feverish, but she had a good night's rest. There was a small discharge thro' the canula of tolerably white pus, but of an intolerable smell. The canula seem'd to us to be much clogg'd with sloughs; and the stench made us suspect a collection and lodgment of these sloughs behind the canula. We resolv'd to put in the canula above described; and as there was a necessity of dilating, in order to introduce it; we agreed to take the advantage of this dilatation, to try to discover by the crooked forceps, which I had brought with me, if there might not be a stone to be extracted, or at least some one of these excrescences, and to break or crush such as we should not be able to draw, that they may fall off by suppuration.

I executed this trial on the 8th day. The dilatation was made between two grooved soundes, as it is done in the greater apparatus between the male and female conductors. I found no stone as yet, but brought away clusters of the tops of fungus's a specimen

cimen of which appears in Plate I. Letter *F*, I crushed the rest of the excrescences, and placed the large canula.

Experience has shewn me, that this bruising of the fungus's of the bladder is more painful and dangerous than possibly is imagined. They are far from being of the same nature with the polypus of the nose, which is pull'd out with little or no pain, and without any bad consequence. The fungus's of the bladder have more consistence, more solidity, and for that reason more sensibility. Accordingly, after this last operation, the patient was seized with a violent fever, which carried her off in two days. I open'd her body, and found the bladder in the condition represented by the figures, and their explanation.

This observation made me think, that if I met with a parallel case, that is, a patient with fungous excrescences in the bladder, distinctly characterized, and accompanied with pains and excessive hæmorrhages, which render the palliative cure useless and unsuccessful; and if he had a constitution and courage proper to make me hope for success from a great operation; I would find a way to attack the excrescences with a cutting instrument, the operations of which are much surer and less painful than any other method. Practitioners advise to suppurate such of these excrescences, as the fingers cannot reach, that is, those which can neither be tied nor cut. But how can one bring such sensible parts to supuration? we have no ointment that can raise a supuration in a sound part. Fungus's are a sort of vegetation, which, tho' preternatural, are still living,

and, in some measure, found parts: how then are they to be disposed to suppurate? it must be either by pulling them out, or by crushing them, as we have done. But seeing this operation is dangerous, an instrument should be contrived, which might be conveyed to the bottom of the bladder, like the forceps; and which might at the same time be able to cut these inaccessible excrescences, or the greatest part of them at least; the remains of which being cut open, would thereby acquire the necessary dispositions to suppurate, which are indicated for the cure. For this purpose it was, that about that time I contrived the cutting forceps of Plate III. the use of which will be sufficiently declared in the explanation of the figures. I did not intend to make this instrument public, until I had used it on a living body: but, as no opportunity has offer'd since the year 1735, I thought I ought not to delay its publication any longer; to the end that, if some other person met with this opportunity before me, he might profit by the reflections, which the preceding observation occasioned me to make.

*Explanation of the Figures.*

Plate I. The bladder of the widow Néel, full of excrescences.

*AAA*, The bladder.

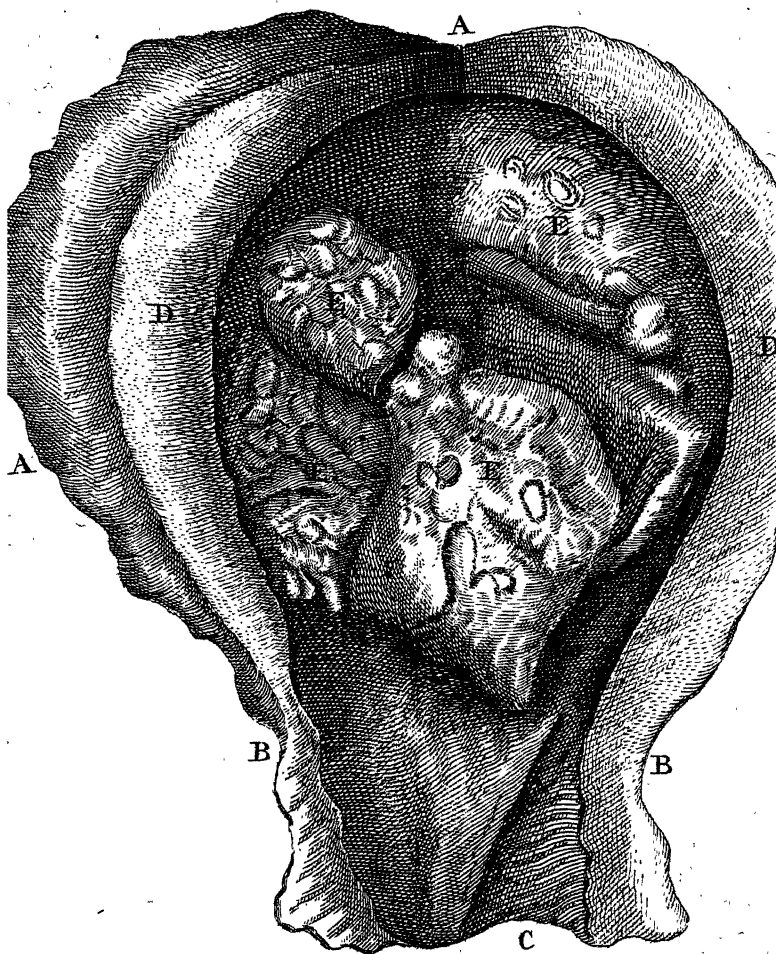
*BB*, Its neck.

*C*, The incision of my lateral operation.

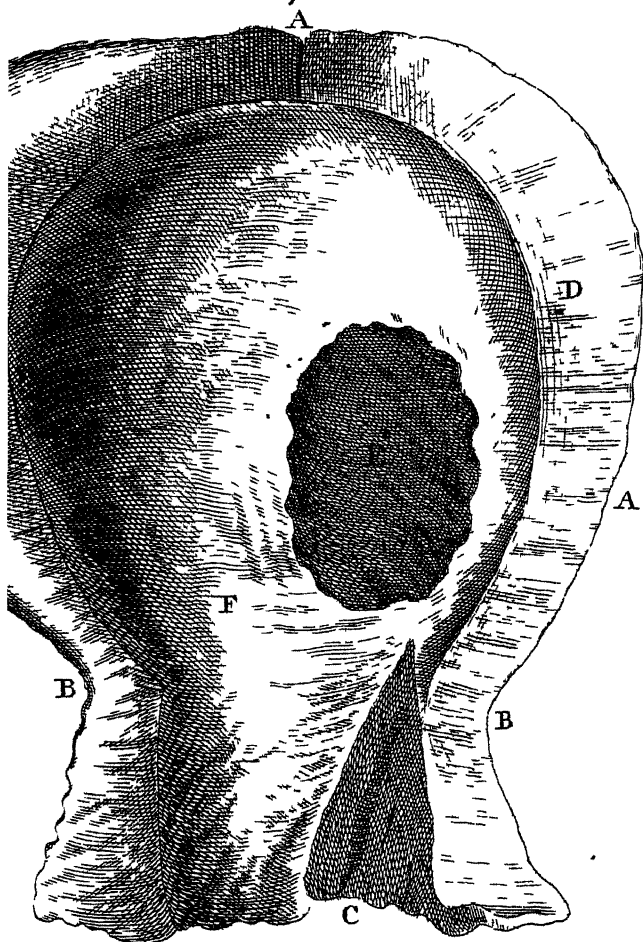
*DD*, Thickness of the coats of the bladder.

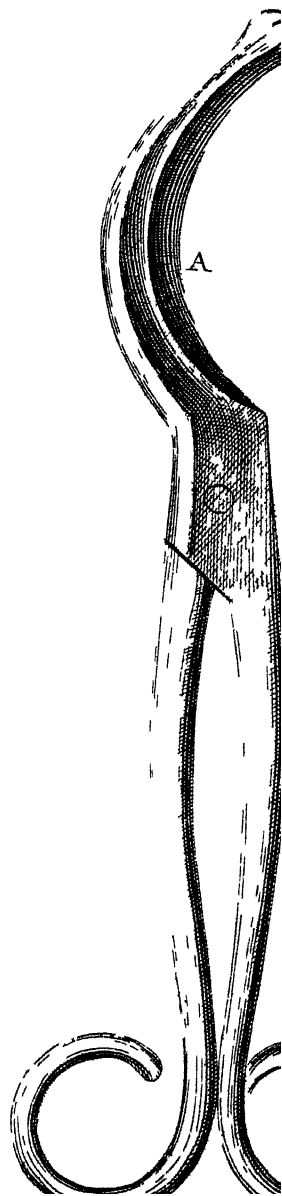
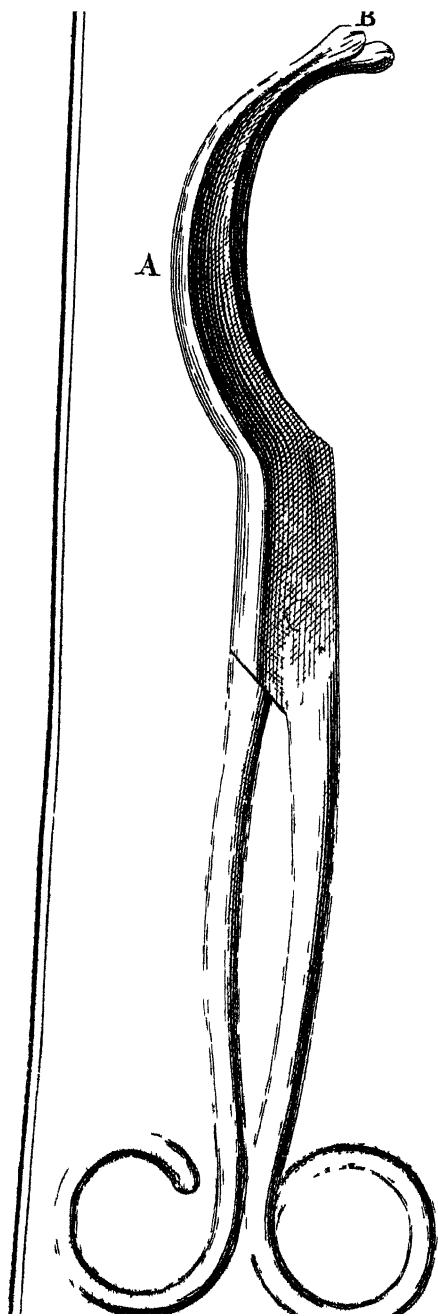
*E*, Remains of the excrescences, which were not extirpated, and seem round or regular, because their  
tops



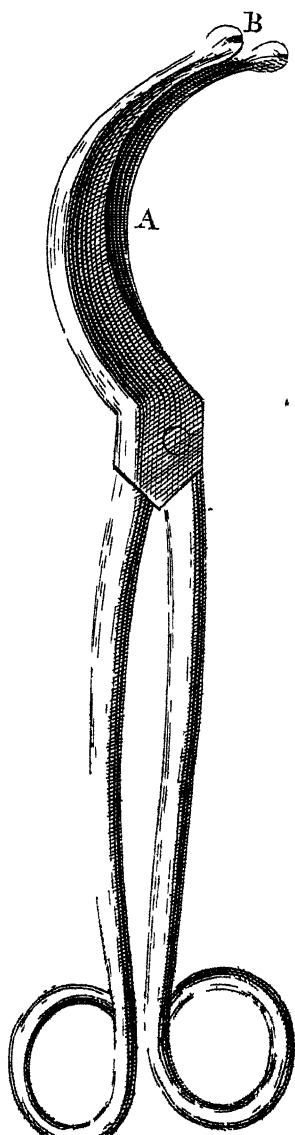


Pl. II. p. 299.

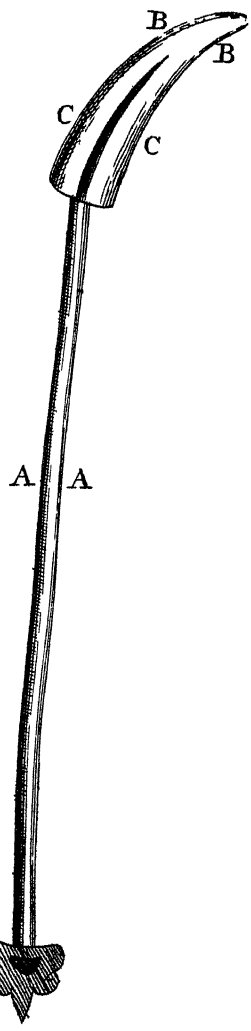




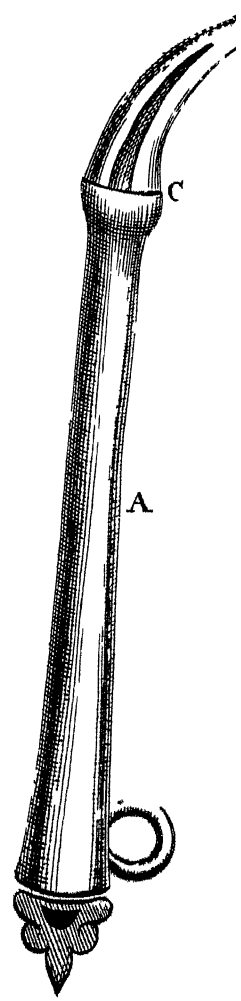
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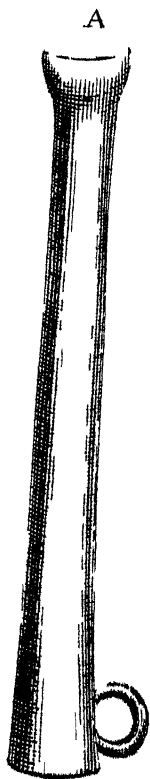


*Fig. 3.*



*Fig. 2.*

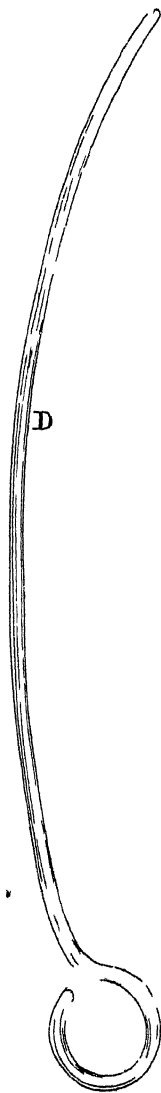
Pl. IV.



*Fig. 4.*



*Fig. 1*





tops ending in clusters were taken off by the forceps.

*F*, The shape of these tops of the excrescences.

Plate II.

The same bladder, after removing the excrescences, in order to shew their common root *E*.

*F*, Very considerable fleshy fibres, which surround this basis, and were confounded in the scirrhus substance. It was this scirrhus basis, that I had touch'd with the staff, and which I took for a hard body. The difficulty of touching it arose from the necessity of passing between two excrescences.

*DD*, The great thickness of this bladder.

Plate III.

Cutting forceps or scissars, to cut the excrescences of the bladder or *uterus*, which are inaccessible to the fingers.

*A*, The bend of this instrument on the flat of its blades.

*B*, Buttons, which terminate each blade, and are at some little distance from one another, even when the blades are closed together: that these ends might neither prick nor pinch the coats of the bladder.

Plate IV.

Fig. 1. The new canula.

*A*, The neck, which is to be embraced by the neck of the bladder.

*B*, The swelling, which is to be within the neck of the bladder.

*C*, The head, which is to be in the cavity of this organ, together with its wide openings.

*D*, The style or sound of this canula.

Fig. 2.

The same canula improved, inasmuch as its end *B*, which I name *introduetor*, is screw'd on the canula *A* at *C*, and is unscrew'd by means of the structure of this introduetor.

Fig. 3.

The introduetor separated from the canula.

*A*, A wire or rod of steel, which supports the end of the introduetor, and serves to unscrew it from the canula.

*B*, The extremity of the introduetor, which ought to be made of silver.

*CC*, Elastic steel plates or blades. These plates have on the inside of their edge a female screw, which enters on a male screw of the outside of the end of the canula. Their springiness makes them separate when the introduetor is mounted on the canula; and by this widening asunder they leave the openings or eyes of *d, d*, Fig. 1, 2. But when they are unscrew'd, they close together, as appears in Fig. 3, whereby this end becomes slender enough to pass thro' the canula, thro' which this part of the instrument is drawn out, when the canula is placed in situation; which is the intent of this structure; for, by this means the outlet becomes larger, and the excrescences cannot be fretted.

Fig. 4.

The canula stripp'd of the part above described.

*A*, Its funnel or tube and wide straight orifice: wherein  
consists

consists the improvement of this last canula, which I had principally in view in the rectification of the

XLVI. *An Account of the Cinnamon-tree,*  
by Mr. W. Watson, F. R. S.

To the Royal Society.

Gentlemen,

Read Nov 21, 1751. **I** TAKE the liberty of laying before you a specimen of the bark and wood of the cinnamon-tree, nearly of the length and size of an ordinary walking-cane, transmitted from our worthy member Mr. Benjamin Robins, now in India, to Dr. Letherland, who was so obliging as to put it into my hands for your inspection. And, in order to convey to you at the same time a yet more perfect idea of the tree itself, there accompanies it a small branch of this valuable plant from my own *hortus siccus*.

Cinnamon, in the state now before you, is a great curiosity, and seen in Europe at present extremely seldom. Clusius tells us, that he saw two specimens of it. Anciently indeed it was often brought in this manner, *viz.* with the bark surrounding the wood; and it is believed by authors of very great credit, that the wood, not divested of its bark, as we now see it, or the bark stripped from the wood, was called by different appellations. And notwithstanding the various controversies, which have arisen in endeavouring

ing to fix properly these various terms, it did appear to the late Mr. Kay, that our cinnamon, the cinnamon of the antients, and the *cassia lignea* of the antients, were quite or nearly the same thing; and that they only had their difference from the soil, in which they were produced, or from the circumstances under which they were brought. Thus the younger branches of the tree with their bark covering them were called by the Greek writers *κινναμόμων*, *cinnamomum*, and sometimes *ξύλοκασία*, or *cassia lignea*; but when they were divested of their bark, which, by its being dried became tubular; this bark was denominated *κασία σύριγξ*, or *cassia fistula*. But as, in process of time, the wood of this tree was found useless, they stripped the bark from it, and brought that only, which custom prevails at this day.

Both Theophrastus and Pliny mention a very odd, and most undoubtedly a fabulous account of the manner of separating the bark from the wood. They say, that it is cut into short pieces, and sew'd up in a fresh hide; and that then the worms produced by the putrefaction of the hide destroy the woody part, and leave the bark untouched.

However the cinnamon, or *cassia cinnamomea* of Herman, the *cassia lignea*, and *cassia fistula* of the ancient Greek writers might approach near each other, they were applied by the moderns to very different substances. By cinnamon is now always understood that only produced in Ceylon; by *cassia lignea*, the cinnamon of Sumatra, Java, and Malabar, much inferior, in every respect, to the former, tho' nearly agreeing therewith in appearance, and not at all woody, as the appellation seems to insinuate; and

and by *cassia fistula*, a fruit not described or used by the ancient Greeks, and agreeing therewith in no one particular, only that both are vegetable productions : great care should be taken therefore, that this confusion is not productive of error.

Burman in his *Thesaurus Zeylanicus* takes notice of his being in possession of nine different sorts of cinnamon of Ceylon ; the most excellent of which is that, which is called by the inhabitants *Rasse Coronde*, and is what is most usually brought to Europe.

What we now call cinnamon, is only produced in Ceylon, of which the states of Holland are in possession ; and so jealous are they of this tree, which affords so valuable an article of commerce, that the fruit or young plants are forbidden by an order of state to be sent from thence, lest other powers might avail themselves thereof. And this they have been hitherto successful enough to keep to themselves ; tho' in Ceylon, according to Mr. Ray, the cinnamon-tree grows as common in the woods and hedges, as the hazel with us, nor is of greater esteem with the inhabitants than other wood, but is used by them as fuel, and applied to other domestic purposes. I am apprehensive, that the prohibition of sending cinnamon-trees from Ceylon is of no long standing, as Paul Herman, who resided there some time, and was after his return chosen professor of botany at Leyden, tells us, in his *Hortus Lugdun. Batavus* published in 1687, that he sent several of these trees to some considerable persons in Holland, and that they continued also as well in the gardens of others, as in his own, for two or three years, and were kill'd by a severe winter. I am very credibly informed,



informed, that three of these trees in pots were presented to the late King William, by whom they were placed in the garden at Hampton-court, and were intended to be sent to Jamaica, as a country proper for their increase, under the care of the earl of Inchiquin, who was then going thither governor. But for want of attention these trees were left behind; and as the knowledge of hot-houses, as we now see them, was unknown, and the state of gardening otherwise extremely low, these invaluable trees were suffered to die here; whereas had they been planted in some of our islands in America between the tropics, in all probability before this time we might have been supplied from thence, and large sums been annually saved to the public, as great quantities of cinnamon are consumed in diet and medicine. I am,

Gentlemen,

Your most obedient humble servant,

London, Nov. 21, 1751.

W. Watfon.

XLVII. *Observations and Experiments upon animal Bodies, digested in a philosophical Analysis, or Inquiry into the Cause of voluntary muscular Motion ; by Charles Morton, M. D. F. R. S.*

Read Dec. 5, 1751. **T**HE paper proceeds in the following order:

*The Problem*, or question proposed.

*Observations and Experiments*, illustrating the structure and use of the parts concerned.

*Two Lemma's*, with demonstrations concerning automatic or involuntary motion.

*Observations* proving, that the sensations, of which we take cognizance, are merely relative.

*Observations* proving, that the will has a power over sensation universally, to render it more or less acute.

*Solution*, or answer to the question, necessarily arising from the preceding facts.

*Some short scholia.*

*Problem.*

A muscle being given in its natural state, in a living animal body, it is asked how, or by what mechanical means, that muscle contracts, and is again relaxed, at the command of the will ?

*Observations illustrating the structure and use of the parts concerned.*

Every muscle of an animal body is observed to be an instrument composed of fibres or lesser muscles,

Q q

which

which are joined together every-where, by one common membrane or substance, called from its appearance, cellular. This substance, when it arrives at the surface of the muscle, becomes uniform, and makes one intire sheath for the whole muscle, or bundle of fibres, and renders it distinct from others.

The constituent fibres in many muscles are observed to be partly fleshy, and partly tendinous; the one changing, or being continued, into the other, for the conveniency of insertion and motion. But the observation is universal, that the fleshy fibres alone contract in muscular motion, and that this contraction is always wave-like, or in alternate curls, from one extremity to the other of a given fibre.

We constantly observe, in every muscle, numerous arteries, veins, and nerves. These are generally distributed together, or in the same course, by means of the connecting cellular substance, into every point of the fleshy fibres. Injections, and the knife of the anatomist, have follow'd them a great way, and reason completes the distribution, since you can nowhere wound the flesh of a muscle, but it shall bleed, and witness a sense of pain.

Therefore there is a circulation of blood, throughout the whole fleshy substance of a muscle: and further the muscle feels in every part.

In a living animal, if you tie the artery and vein, which principally belong to a given muscle, that muscle is disabled from acting at the command of the will. Steno, a Danish anatomist of the last century, performed this experiment upon the descending aorta, and thereby took away the use of all the lower limbs (*vide Bergerum*, p. 296) at once, and restored them

at

at pleasure. Late anatomists have tried it upon lesser vessels, with the same constant success. (*Vide Albini histor. muscul. p. 19.*)

In a living animal, if you tie the nerve, that supplies a given muscle, that muscle is disabled from acting at the command of the will. This experiment is distinctly mentioned by Galen in his treatise on the muscles, and is approved by the trials of later anatomists. (*Alb. p. 19.*)

From these two experiments it is clear, and generally agreed upon, that, in order to the performance of voluntary muscular motion, besides the particular structure, there is required an absolute freedom of the blood-vessels, and the nerves.

Muscular motion is observed to be voluntary, and involuntary. Of the first kind are almost all the muscles of an animal body; of the latter, the only complete instance is the heart. The first seems more complex than the latter, since, besides the motion, it implies an additional act of the will. Effects, that are less compounded, ought naturally to precede effects, that are more; these receiving light from the former, where both are homogeneous. For this reason, I have placed here two lemma's relating to automatic, or involuntary motion.

### *Lemma I.*

The heart, in its natural state, in a living animal body, being given, its contraction proceeds solely from, or is mechanically caused by, the warm blood, flowing into and filling its fleshy substance in every part.

If this be denied, let the body of an animal be taken quickly after death, and let a warm mild fluid of any kind be injected gently into the heart, so as to fill it. When this is done, we shall see the heart quicken and contract, as in the life of the animal. This experiment was first distinctly mentioned by Peyer a Switzer (see a small treatise of his, printed *anno* 1682, at Amsterdam, and intituled, *Miraculum anatomicum in cordibus suscitatis*) and is now known to every anatomist. But if this effect is thus constantly produced soon after death, how much more, when the animal is alive? And if, by the introduction of any common fluid, with the bare addition of a warmth cognizable by our senses, how much more by the introduction of the living blood, an inimitable and wonderful fluid, and the immediate subject of the vital warmth?

If therefore it is granted, that we ought not to admit more causes of natural things than are real (and present for the occasion) and sufficient for explaining the appearances (*a*), and we must grant a rule, whose use is so obvious in the Newtonian, which is the philosophy of nature; we shall, I say, also grant, that the contraction of the heart, in its natural state, in a living animal body proceeds solely from, or is mechanically caused by, the warm blood, flowing into, and filling, its fleshy substance in every part. Which was to be proved.

*Corollary.*

The subsequent relaxation admits no difficulty: for if the blood is the immediate mechanical cause of the contraction, when the blood is removed, the effect ceases.

*Lemma*

A muscle of voluntary motion, in its natural state, in a living animal body, being given, it will contract by the introduction of a warm mild fluid, into its fleshy substance in every part.

If this be denied, let the body of an animal be taken quickly after death, and the crural artery be pierced, and a warm mild fluid be injected into it: we shall then see the muscles, to which the artery belongs, quicken and contract, as if the living animal moved them. This experiment was known to Mr. Cowper, and is confirmed by Albinus (see *Hist. Musc.* p. 21.)

But if this effect is constantly produced soon after death, how much more when the animal is alive?

Therefore a muscle of voluntary motion, in its natural state, in a living animal body, will contract, by the introduction of a warm mild fluid, into its fleshy substance, in every part: Which was to be proved.

But here it may be objected, with some appearance of reason, that there is a warm fluid, the living blood, in every part of the fleshy substance of all the muscles, during the life of the animals; and yet it is a fact, that no muscle of voluntary motion contracts, but at the command of the will, morbid cases excepted. This objection comes close to the original question, and however reasonable it may seem, will quickly vanish before some common observations concerning the objects of sense in general, and their manner of operating upon the different organs, so far as it universally agrees.

We must first beg leave to make an easy postulat-  
um, viz. that the nerves are the immediate instru-  
ments of sensation, though they are differently or-  
ganized for the different senses.

*Observations, proving that the sensations of which we  
take cognizance are merely relative.*

It is a certain fact, that, in the several senses, the  
proper objects being suppose present, the sensation is  
intirely relative ; or, in other words, that the presense  
of a powerful object always obliterates the present  
sensation of a weak object ; and that the constant ha-  
bitual presense of any one object, in the same given  
degree, produces no sensation at all.

Thus we observe, that the light of the sun extin-  
guishes the light of the stars ; a stronger taste covers a  
weaker ; the sound of a drum drowns an ordinary  
human voice ; itching is banished by smart and pain ;  
a weak scent, by one that is strong ; cold, or a less  
degree of warmth, by heat, or a greater degree of  
warmth ; and universally, our daily experience de-  
monstrates to us, that every organ of sense, made  
familiar to a given degree of its object, affords no  
manner of sensation of the object in the given degree.

Thus it fares with the warm blood, which has  
constantly flowed through the whole minute sub-  
stance of every muscle of voluntary motion in an animal  
body, from the time of their formation, or unfold-  
ing in the womb. And it is highly probable, that  
the quickening of the child in a woman is no other  
than the completion of that state, in which the blood  
begins freely to flow through, and to affect the in-  
struments of voluntary motion ; and till it becomes  
familiar

familiar to them, produces those frequent shudders, or general muscular contractions in the whole frame of the fœtus, which for a fortnight or more are the constant signs, that it has now obtained an animal life.

And here arises an apparent difference, though it will be found the greatest uniformity, between the muscles of voluntary and those of involuntary motion; and namely the heart; which being appointed to protrude the vital fluids during the life of an animal, has a short alternate remission of its contracting cause; and is thereby render'd capable of admitting a constant and necessary supply of labour and stimulus together, without any force, or contradiction, to the natural order of the whole.

It follows undeniably from what has been said, that if we can prove, that a given muscle of voluntary motion, does really feel an increase of the familiar warmth of its contained blood, or an equivalent, to rise and fall instantly at the command of the will, we shall then duly account for the subsequent motion. Or, more particularly, if we can prove, that the will has a direct power of heightening, increasing, and rendering more acute, the sense of any nerve, distributed to a given muscle, the same familiar positive degree of warmth in the contained blood will, to this more acute sense, appear to be proportionably heightened and increased, and the muscle (by lemma 2) will instantly contract, and continue in that state during the action of the will; allowing for a small feebleness, that will gradually arise from the gradual exclusion of the contracting cause, and from the blunting of this more acute, and, as it were, new sensation; which yet, as we see, may  
be



be proportionably compensated, by the will, for a time, even to the destruction of the nerve, the blood-vessels, and indeed the whole organ, by a mortification, which has been known to succeed a long muscular contraction.

*Observations, proving, that the will hath a direct power of rendering more acute the sensations of the nerves universally.*

We know from daily experience, that the will hath a power over all the organs of sense, to heighten, or render acute, and again to relax them, their proper objects, in a reasonable degree, being supposed present. And the same experience teaches us, that this power is greater or less, according to the more or less frequent use and exercise that is made of it. For it is obvious to every one, that any sound man is able to feel, to taste, to smell, to hear, and to see, more accurately when he pleases. And it is equally obvious and certain, that any one of these five senses, being exercised, with an uncommon degree of attention and industry, either from choice, or from necessity, arrives at an uncommon degree of accuracy, and perfection. Indeed it is intirely from use and exercise, that a child learns to distinguish at all between the several objects of a given sense, or, which are the same, between the several degrees, or modes, of its proper object.

All these particulars, being demonstrably true of every sense, that we can directly examine, the inference is very fair to the single sense (*Leem. 2.*) that we cannot directly examine; and, in truth, the induction in this case, is but one step below a complete experimental demonstration.

It

It appears therefore, that the will hath a direct power of heightening, increasing, and rendering more acute, the sense or feeling of a given nerve, dispersed throughout the whole contracting substance of a given muscle, with all its gradations of accuracy and perfection. by repeated use and exercise.

*Solution, or answer to the problem.*

It follows therefore, that, a muscle being given, in its natural state, in a living animal body, the blood, which is present in every part of its contracting substance, and which, in effect, to the sense of the given muscle, (which is occasionally render'd more acute) puts on an increased heat, and again lays it down at the command of the will, is the immediate mechanical cause, by which the muscle does instantly contract, and is again relaxed, at the command of the will.

Therefore, a full solution is given to the question proposed : which was to be done.

*Corollary 1.*

Hence it appears, that muscular voluntary motion is performed merely as a sensation (*a*), extremely acute, and under the nicest management of the will : which explains its velocity in a great measure.

R r

*Corol.*

*Corol. 2.*

Hence it appears, that the Galenic distinction of nerves, into nerves of sensation and nerves of motion, which greatly puzzles physiology, has no real foundation in an animal body.

*A short scholium.*

The solution, that is given to the problem, may be assumed in a philosophical synthesis, and the various appearances may thence be announced, as well in natural as in morbid cases; which again may be subjected to a strict examination. Some trial has been made of this, and a surprising agreement found: but the detail must be omitted. In the course of this inquiry, every foreign disquisition is industriously avoided, and such at this time would be a further question, Why blood, in a certain, or apparent, degree of heat, contracts a muscular fibre?

The business of natural philosophy is, to observe, and to note down facts, that are constant; and singling out those that are similar, to collect their proper universal, by a fair and regular induction; and to acquiesce in this, till a new collection of constant and similar facts affords an higher universal, and leads nearer the first cause.

. October 16, 1751.

XLVIII. *An Account of the Eruption of Mount Vesuvius, from its first Beginning to the 28th of October 1751, in a Letter from Mr. Richard Supple, communicated by Mr. Benjamin Wilson, F. R. S.*

Read Dec. 19, 1751. ON the 23d of September 1751, at 11 in the morning, there was an earthquake, which was felt more or less, as we were nearer or farther off from the mountain. It lasted near 2 minutes very sensibly in the city of Naples; but most so in that part, which lies nearest the mountain. We make no doubt but it was at this instant, that the eruption of burning matter or lava was made.

The mouth, from which this lava issued, was discovered on the 24th in the evening, as it run out, and down into a deep valley between the canal of and the tower of Launomiado. The lava did not appear on the face of the valley, which it had just filled, till the 26th in the morning. Then it took a serpentine course through several antient chanel, where the lava had run, and appeared on the lands.

On the 27th in the morning, the lava having run two miles from the mouth whence it issued, it advanced with a breadth of 300 feet, and 30 deep, and pretty slow.

From this frightful mass of burning matter there issue two principal streams of lava, that have filled two valleys, which are near 200 feet deep. One of those

those streams advances about 3 feet and a half in a minute, and the other about 3.

The first has advanced already one mile into the plain, which has a descent into that of Siena, between the tower of Launomiade and Scoffata, and moves on with a stream of 100 feet broad, and about 6 feet deep. It has actually gone 4 miles from its head or mouth. I approached within 10 feet of this river of fire, and put a branch of a tree, just cut off, so near it, as to be distant about 3 inches, which it instantly burnt without any smoke. I had my face changed yellow with the smoak or steam that issued from the lava; and this smoak was so violent, as to take away my breath, and made me apprehensive of losing my life.

The other lava flows directly towards the village of Launomiade, and is still advancing. All the inhabitants have abandoned that village, fearing it may share the fate with Herculaneum and Stabia. The main stream ruined in the night, between the 27th and 28th, a tract of half a mile. It has divided itself into 12 branches, according to the situation of the land, and these again have united, and become one stream.

The lava seems to be much more charged with metals and fire than any of the former; and the eruption appears to send out 10 times more matter than that in 1737: but that was much more frightful, from the continual thunder it made, and by the burning matter that it threw to a prodigious height; and which afterwards run down to the foot of the mountain, leaving behind it a ridge of fire, which, during the night, had an effect as surprising as terrible.

If the first branch continues, it will cross the high road from Naples to Salerno, and throw itself into the river Sarno, and change its course, and may go as far as Stabia, as it did in the reign of Titus Vespasian; though this buried city is twelve miles from the top of mount Vesuvius.

Marfeilles, 7 Nov. 1751.

Richard Supple.

XLIX. *An Account of the Eclipse of the Moon, which happened Nov. 21, 1751; observed by Mr. James Short, F. R. S. in Surry-street.*

Read Dec. 19, 1751. **T**HE weather was exceedingly tempestuous, and the sky pretty much overcast with clouds, so that the following times cannot be depended upon to less than 2 minutes.

			1	11
Penumbra very visible at	.	.	7	58 0
Beginning of the eclipse at	.	.	8	6 0
End of the eclipse at	.	.	11	6 0

The quantity of this eclipse seemed about the middle to be larger than according to all the tables; but its quantity, tho' the air was then exceedingly clear, could not be measured in the micrometer, because of the high wind; nor could the moon's diameter be measured, for the same reason.

Transit

Tranfit of the moon over the meridian.

Preceding limb passed the meridian at	12 5 18
Subsequent limb passed the meridian at	12 7 50
The sky was at this time exceedingly clear.	

Mr. Pound observed a similar eclipse at Wanstead, just two Sarotic periods before this, and has described it in the *Philos. Trans.* N. 347, p. 402. and makes the following remark, " This eclipse is  
 " the more considerable, as happening very near  
 " the moon's perigee, and therefore useful to verify  
 " her anomaly ; as also to limit the greatest diameter  
 " of the shadow of the earth, and consequently the  
 " parallax of the moon. This may be very properly  
 " compared with that of the 19th of October 1697,  
 " whose middle was at 7<sup>h</sup> 41' *p. m.* at London, and  
 " the quantity the same as now."

Here follows a computation made from Dr. Halley's tables by Mr. John Catlyn of Guy's Hospital.

Beginning at	8 18 44
Middle at	9 41 55
End at	11 5 6

But if an allowance is made for the errors in the moon's motion, when she was in similar circumstances in the month of November 1733, the above times of this eclipse may be marked with the following numbers.

Beginning at	8 14 0
Middle at	9 37 30
End at	11 0 30
	I must

I must add to Mr. Pound's remark above, that this eclipse happened nearer to the moon's perigee than that, which he observed in the year 1715, and therefore more proper for verifying the moon's anomaly, and limiting the greatest diameter of the shadow of the earth.

L. *A Letter from the Reverend Father Augustin Hallerstein, of the Society of Jesus, President of the Astronomical College at Peking in China, to Dr. Mortimer, Sec. R. S. Translated from the Latin by Tho. Stack, M. D. & F. R. S.*

S I R,

Pekin, Sept. 18, N. S. 1750.

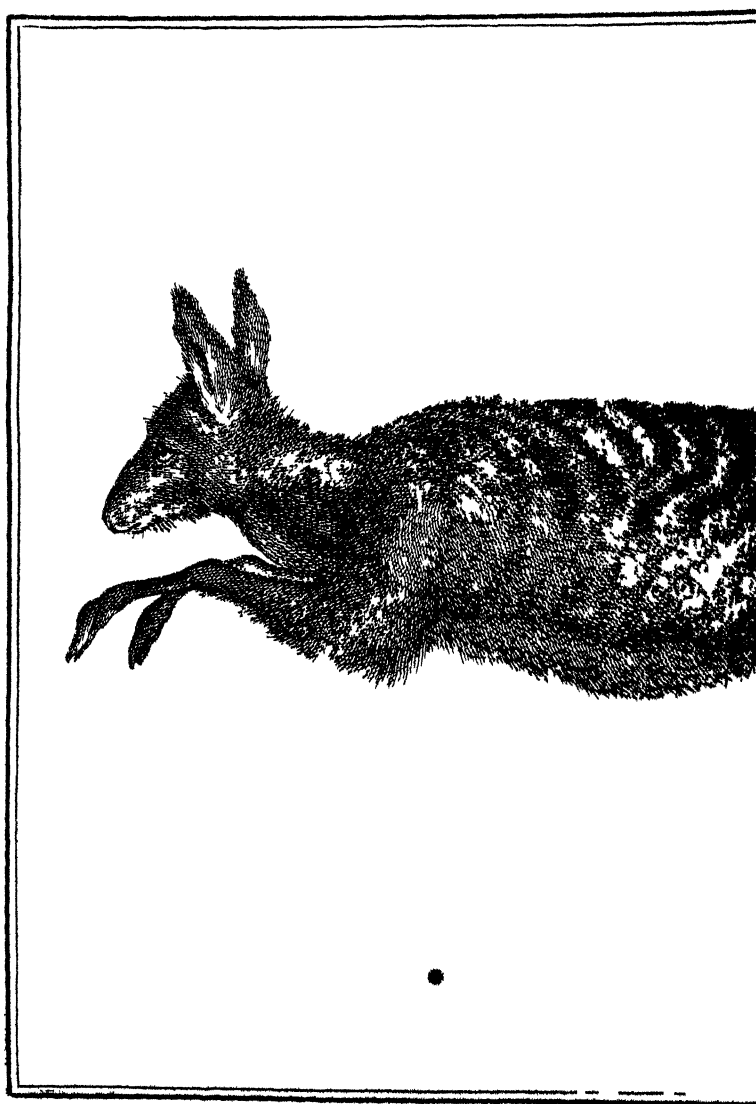
Read Dec. 19, 1751. **Y**OUR letter of Feb. 5, 1746, we duly received, and answered as well as the shortness of time allowed us would then permit. In the year 1749, a volume of the *Transactions* was brought to us, for which we return'd thanks to your illustrious Society, and now repeat them in the most cordial manner. As far as our condition here, and the iniquity of the times, will permit, we will never be ungrateful for so great a favour. What we had then ready, and thought might not prove disagreeable to the Royal Society, we sent directed to you, Sir; viz. two Chinese volumes, one of which contains logarithmic tables, formerly translated into Chinese by some of our Society; and the other luni-solar tables constructed from the numbers and measures of the

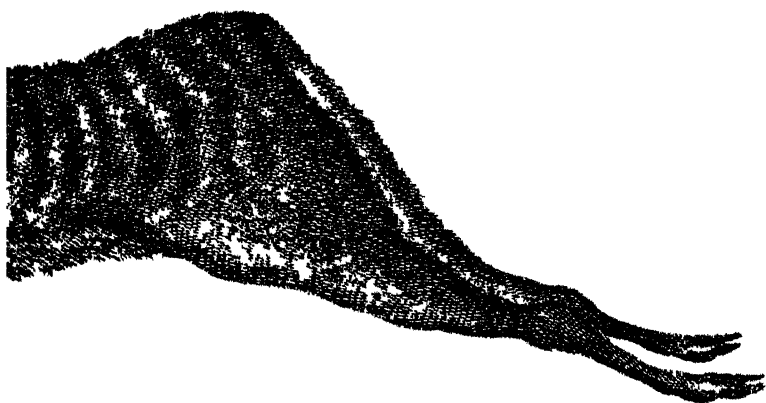


the illustrious Newton, which we use at present in our astronomical observatory (or tribunal as we call it) for calculating ephemerides and eclipses. In this packet we now send our astronomical observations for 1746 and 1747; and next year we will send those of 1748 and 1749. And we are encouraged so to do, by the Royal Society's and your great humanity; as you have been pleased to think those for 1740 and 1741 not unworthy of a place in your *Phil. Transf.* that treasury of all sorts of erudition.

From the year 1741 to 1746, we made but few observations. For my predecessor Father Ignatius Kogler was then broken with age, and I was wholly taken up with learning the Chinese language and letters. Yet possibly even these few observations may appear some time or other, with a long series of others, which the aforesaid father made from 1718 to 1745, and set down in loose papers; which I have brought into order, and wrote into one volume, in the order of years and planets; and wish I had leisure to transcribe that volume. However, both he and I went as far as we could. For, to say it by the bye, those bulky machines of our royal observatory here, tho' magnificent, and of solid brass, do not come up to the accuracy of the present time. And the astronomical apparatus of our house, that we can depend upon, almost intirely consists of a micrometer, a pendulum-clock, and a two-foot quadrant. To which may be added a transit-instrument, which we have received a few days ago, by the courtesy of Dr. Antonio Ribeyro Sanchez, a Portuguese, and first physician to the court of Russia: to which if a good quadrant, such as are made now, were added, then we might attempt greater things. For, let the observations









tions made with only a micrometer and pendulum be ever so accurate, they are rare, and cannot always be made. In the mean time we will place the transit instrument, and use it as far as its use extends. For a quadrant we apply to the court of Lisbon; because we have it not in our power to purchase one. And indeed, the report of the wealth of the Jesuits at Pekin is a mere fable.

We have not yet had the good fortune here at Pekin to see an accurate figure of the male musk animal: the figure here inclosed is that of the female; and it is not this, but the male, that is said to bear the musk. This figure was drawn in our house by Father Ignatius Sichelbarth, from a dead animal, as it was brought to us. The Chinese, who have seen the male, say, that it is not much unlike this figure, excepting that it has larger teeth, and sometimes tusks like those of a boar. On some other occasion we will take care to send you its figure. In fine, the Chinese call both the male and female *biam cham su*, which means the *little odouriferous deer* (*damula odorifera*).

We carefully keep the syllabus of other things, of which you desired to be informed; and shall use our endeavours to satisfy you on these heads, and any other that may give you pleasure. As to geographical maps, and plans of cities, it would be very difficult at present either to obtain or make any, but those already published, in Europe, until a more favourable air from this court breathes on us. Last year I and Father Felix de Rocha travelled into North Tartary, beyond that vast wall, which separates (or at least separated) the Chinese from the Tartars: where by the emperor's order, we drew a

S f                      chorographical

chorographical map of the country, into which this our monarch makes an excursion generally every third year, in order to take the diversion of hunting, and keep his court and army in exercise; pursuant to a custom established by his grandfather, to prevent the Tartars from growing enervated by idleness. And yet they daily grow so more and more; and as they are now more effeminate than the very Chinese, it is not without reason that they are under great apprehensions.

I would send you, gentlemen, a copy of this map, if we had been allowed time enough to make it more accurate. The work was indeed pleasing to the emperor, and upon our return he gave us a most gracious reception, and asked us many questions concerning that country. It is one degree in length, and one in breadth, situated between  $41^{\circ} 30'$  and  $42^{\circ} 30'$ . Its western limit is in the same meridian with the city of Pekin, which the Chinese take for the first meridian both in astronomy and geography. The whole country is one continued chain of mountains and valleys, without inhabitants, but full of wild beasts, as deer, boars, bears, tygers. The passages of the valleys are guarded by troops all around, and no body is allowed to pass thro' them.

Chinese vocabularies, which interpret the Chinese words in Latin, or any other European language, are very scarce, and for the most part very defective. Nor is there any one as yet brought to a sufficient degree of perfection, to deserve printing, or the expences attending it. Those which we use the first years after our arrival, were either left by our predecessors, or written with our own hands with infinite labour.

labour. And even these are not of any great use to us, except the first two or three years, to read and understand some easy books of the Christian doctrine composed by our fathers. For, in order to read the more difficult Chinese books, and especially their classics, we make use of Chinese vocabularies, which explain their characters and hard words in the Chinese tongue, but in a plain and easy manner, much as the Latin dictionaries of Stephens, Nizolius, &c. If we could be informed, that such Chinese vocabularies would prove agreeable to you, gentlemen, we could easily send them.

As touching specimens of butterflies, insects, shells, &c. Father Dincarville, a Frenchman, is the most knowing amongst us in these matters; and as he has the care of sending such things to France, he undertakes to send you at the same time specimens of whatever he can procure: and indeed he sent some the last year 1749.

I am the Royal Society's in general, and in particular,

S I R,

Your most obedient servant,

Augustin Hallerstein.



LI. *A Letter from Mons. le Cat, F. R. S. to Dr. Mortimer, Secr. R. S. Translated from the French, by Tho. Stack, M. D. F. R. S.*

S I R,

Rouen, April 3, 1750, N. S.

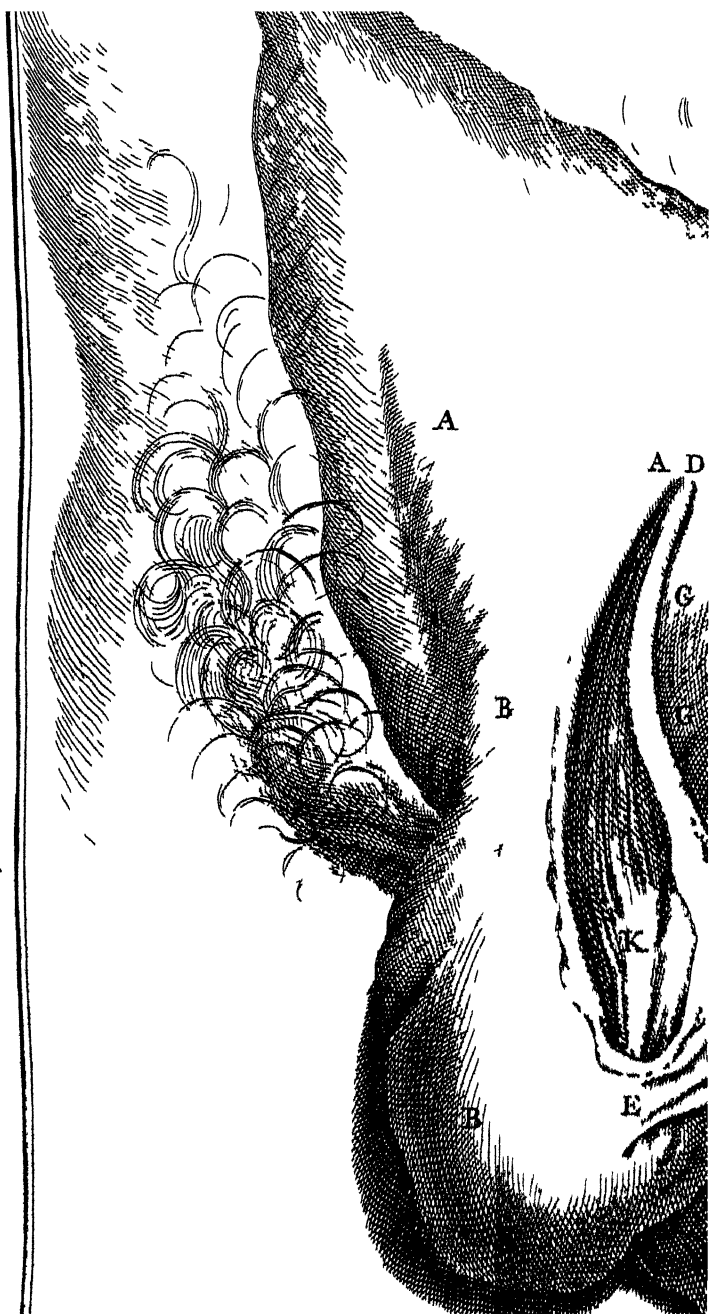
Read Dec. 19, 1751. **I** LOOK on it as a novelty in surgery, to find, 1<sup>st</sup>, hernias by rupture, having nevertheless a hernial sack; 2<sup>dly</sup>, hernias by dilatation, having two very distinct sacks. Wherefore I judged that these observations deserved to be communicated to the Royal Society.

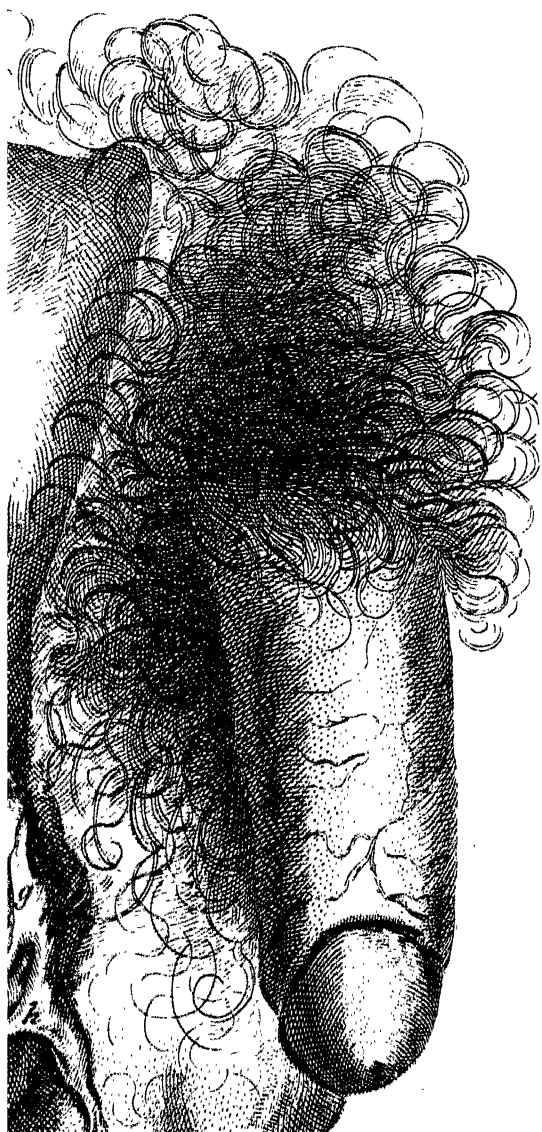
I.

*A hernia by rupture, having nevertheless a sack.*

On the 18<sup>th</sup> of February 1750, in giving a private course of operations to my English pupils, on the body of one Lewis le Clerc, a lad of eighteen years old, a weaver, of the parish of St. Maclou, I discovered the hernia represented in the figure. The aponeurosis of the *musculus obliquus externus AA* ran over the whole tumor *BBC*, and intirely cover'd it. At the anterior and lateral internal part of this tumor was the ring *DE* lengthened into the shape of a perpendicular button-hole; which had nothing to close it but a cellular *lamina*, of which *g, h*, are jags, and which covered all this bag, as being a continuation of the cellular *membrana adiposa*. Through the above-mention'd button-hole appeared the cellular coat, with which the *peritonæum* furnishes the spermatic vessels.









vessels. The intestine occupied the rest of this bag; and at the bottom *BE* was contained the testicle, which consequently had never taken the way of the ring to come out of the belly, as it usually does; but having passed on one side, it had gradually pushed out the *aponeurosis* of the *musculus obliquus externus*; and the intestine having follow'd it, and broke the true *lamina* of the *peritonæum*, they had in concert formed this elongation. At least this is the most natural explanation that I can give of this singularity. That the testicles are originally in the belly, is a fact sufficiently known. I have dissected foetus's, in which I found them therein near the bladder. It is pretty common to feel them in the rings in children; and I have found them there even in lads of upwards of twenty years old.

## II.

*A hernia having two sacks.*

Continuing the above-mention'd course, on the 5th of March 1750, I found in the body of Nicolas Janaux, a batchelor of 48 years of age, by trade a cloth-worker of St. Owen de Longpaon, a rupture with a double herniary sack, the first of which was formed by the expansion of the *aponeurosis* of the *obliquus externus*, as in the preceding observation, excepting that this expansion was only on the outward side, that the ring was in its usual place, that the bottom of the bag formed by this expansion had some empty spaces, where the expansion was wanting. In a word, the bag was neither as complete, nor as thick as that of the foregoing observation; but

on the other hand, there was a second bag, formed as usual by the true *lamella* of the *peritonæum*.

### III.

#### *Another sort of duplicity of the herniary sack.*

Francis le Monnier, coachman, of the Rue St. Laurent, about 65 years of age, had a rupture of long standing, of the strangulation whereof I had already cured him in 1748. Having taken off his truss, in order to get it mended, he was seized with strangulation the 19th of Feb. 1750. After applying all the remedies prescribed in such cases without success, I was obliged to perform the operation on the 21st at eight in the evening. Having laid the bag open in the usual manner, which contain'd a little watry humour in it, I was much surpris'd at discovering within this bag a second bag, or pocket, which could be nothing else, but either a second herniary bag, or an incomplete hernia; that is to say, a portion only of one side of an intestine elongated, and come down thro' the ring. The number of considerable blood-vessels on this pocket, its thickness and fibrous texture seem'd to evince the latter. But first, upon pressing this bag, all its contents return'd into the *abdomen*; secondly, the patient assur'd me, even at the instant, that his rupture had kept up since its reduction in 1748; and I found this bag adhering, not only to the first bag, but also attached by old and strong adherences to the testicle and spermatic vessels; and it was impossible that this state should be the effect of three days of strangulation. However, as the patient might possibly  
have

have deceived me in his account; and as it was dangerous to open a bag which had too near a resemblance with the gut of an incomplete hernia, I came to a resolution, which equally suited the two suspected cases. I separated the testicle and spermatic vessels from this sack, and pushed back this pocket, or second bag, into the belly.

The patient having died on the 9th day after the operation, we found, that the pocket which had given us so much uneasiness, and which I had reduced into the belly, was really a herniary sack formed by the true *peritonæum*; and therefore that the first sack must have been either an interior aponeurotic *lamina* of the abdominal muscles, or the cellular membrane thickened by the long duration of the hernia and its strangulations. The considerable thickness of the true or second sack renders this notion very probable. I say that the first sack must have been formed by an interior aponeurotic *lamina*, and not from an exterior one, like that of the first observation; because, in this operation, I had freed the ring, in my usual manner, above this first sack, and without opening it. Then I passed the grooved catheter over this sack, under the *aponeurosis* or pillar of the *musculus obliquus externus*; and therefore this sack could not be a continuation of this external *aponeurosis*, but that of some more inward *lamina*, or of the cellular membrane of the very *peritonæum*, separated from the true *lamina* by the serosities which we found in it.

To this letter I will add two observations made about the same time,



## I.

*A natural blind duct, being a production of the true lamina of the peritonæum by the rings.*

March 5, 1750, in the dead body of Magdalen Vauchel, wife of Thomas Fermant, 46 years old, I found this duct of the thickness of a goose-quill, be- a production of the true *lamina* of the *peritonæum* stretched out by the rings; of which Swammerdam and Nuck dispute the discovery, and Blancard denies the existence. What made me discover this, was, that its extremity was widen'd into the shape of a bubble as big as the top of a finger, and full of a watery humour. This woman had never had a her- nia, nor even the least tendency towards one.

## II.

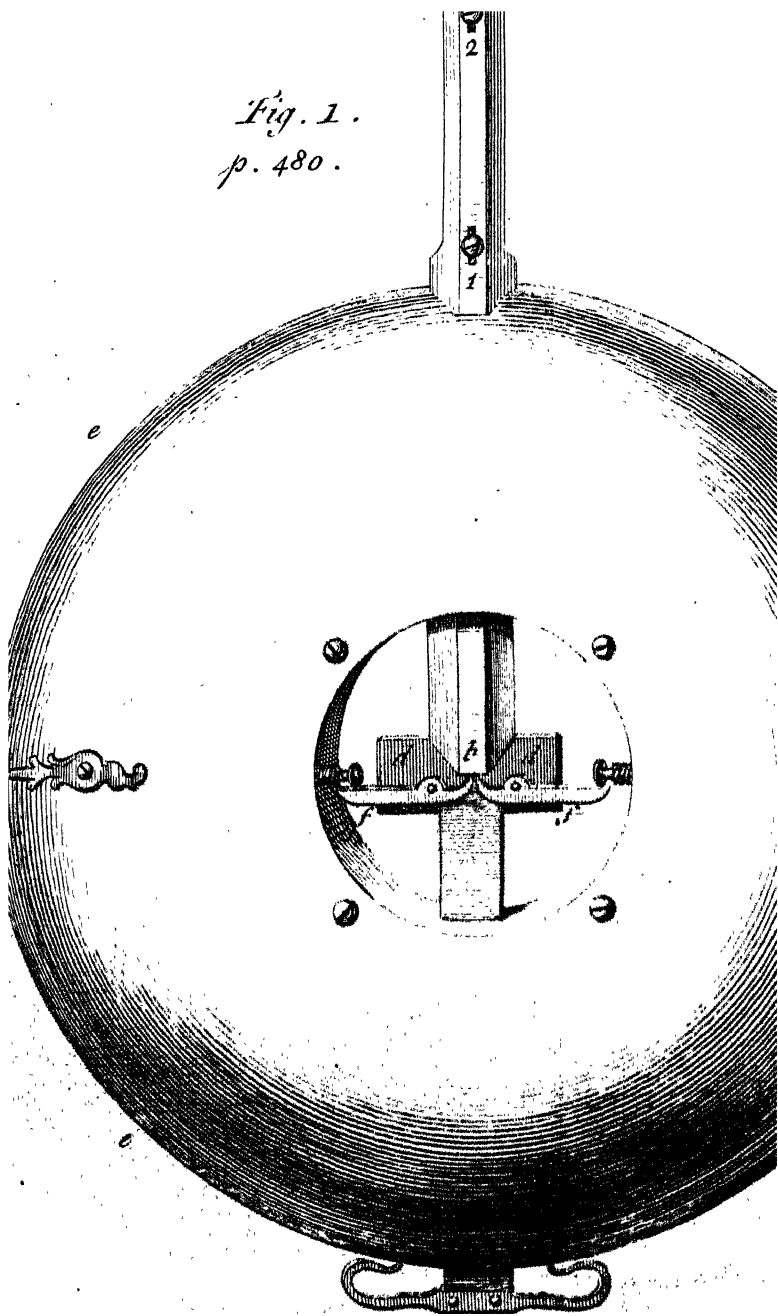
*Strictures and carnosities in the urethra.*

Nothing is more common at this day than to hear people assert, that strictures and carnosities of the *urethra* are mere chimera's; that the bodies of per- sons, who were thought to have these strictures and carnosities, had been open'd, and that none of these had been found. I myself have made this observa- tion, and I inferred thence, that there were *ure- thra's*, in which a *phlogosis*, a fungous inflation gave occasion to the deception, being taken for strictures and carnosities: but if I had drawn this general in- ference, that of all the *urethra's*, wherein these stric- tures and carnosities are thought to be found, not one has any thing in them, I should have been de- ceived, and would now make my recantation.

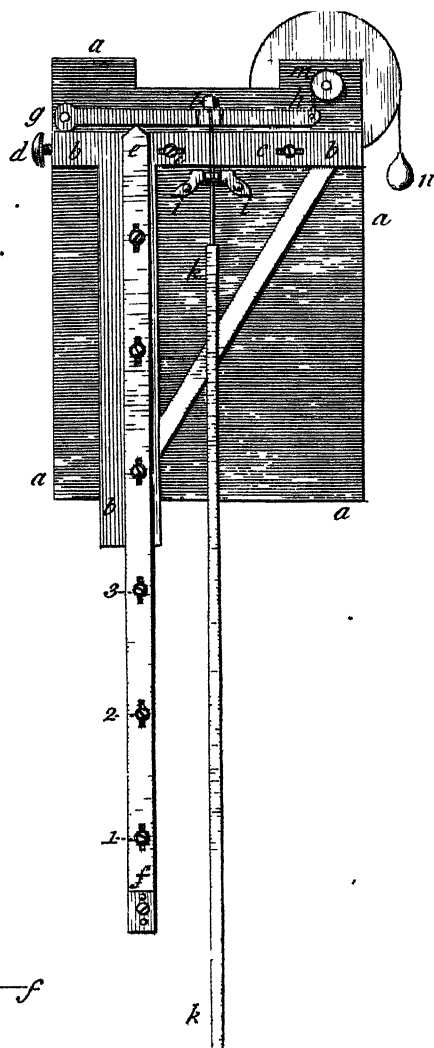
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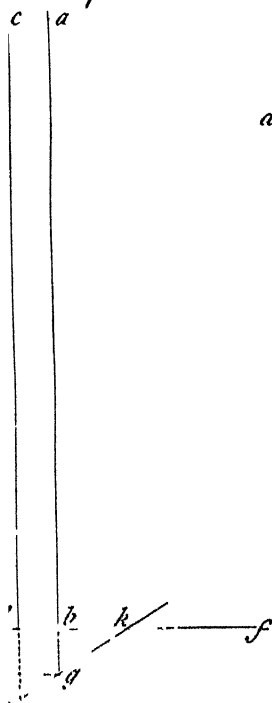
*Fig. 1.*  
*p. 480.*



*Fig. 3 .*  
*p. 486.*



*Fig. 2 . p. 481.*



*Fig. 4 .*

*p. 488.*



One of my boarders preparing to perform the operation of cutting on the dead body of Michael Vassal, a batchelor, aged 45, the sound could not pass; the pupil forced, and made a false passage.

I open'd this canal, and found,

1st. That a simple small stile could not pass into the urethra, by pushing it from the glans towards the prostate; but that it passed, by pushing it from the prostate towards the glans.

2dly. A little before the place, where the bulb becomes less thick, and begins to surround the urethra, that is, about a large finger's breadth from its beginning, there was a stricture intirely like that, which Dr. Willis discovered in the upper longitudinal sinus of the *dura mater*.

3dly. Some few lines lower down was a caruncle, or a fleshy firm bump, of the size of a pea; and below this bump, the urethra was extremely streightened.

4thly. The basis of this carnosity formed a kind of valve, and there I found the false passage, that went into the substance of the bulb.

I have the honour to be,

S I R,

Your most humble, and

most obedient servant,

Le Cat.

LII. *An Account of the Effects of Lightning at Southmolton in Devonshire, by Joseph Palmer, Esquire.*

Read Jan. 9,  
1752.

ON Thursday the 6th day of June 1751, about 3 o'clock in the afternoon, (that day, and some others before, having been extremely hot and sultry, and the wind pretty strong in the south-east) a flash of lightning attended with an uncommon thunder-clap, which immediately followed or rather accompanied it, fell upon the windows and walls of the church and steeple of South-Moulton in Devon, greatly damaging them.

The lightning seemed to divide itself into three parts, one of which struck on the east angle of the south-east buttress of the chancel near the ground, and made a large opening in the same: it likewise very much rent and shatter'd a large stone just above the aforesaid opening, as if done by the force of gunpowder; it split another large stone adjoining, and shiver'd the wall near the foundation, in a very odd manner.

Another part of the lightning took off a slice, about 3 inches thick, of a very large angular stone on the west side of the same buttress, forced inwards a large free-stone window of the church, and greatly shatter'd it (tho' it broke very little of the glass) in so much that it is thought it must be taken down and rebuilt: it then pass'd cross the church, and damaged the north side, enter'd a passage before the vicar's house, which was in a direct line of its course, and beat a stone of the floor to pieces.

A person standing by the south window within the church, at the time when the lightning happen'd, felt a blow cross his foot, as if it had been taken off with an ax; and others near him had strokes in different parts of their bodies, the fire-ball, as they call'd it, passing between them.

Beside this ball of fire, they observed likewise another ball, to appearance, which (after damaging 3 or 4 more large stone window-frames, and making breaches in divers places of those stone frames and south wall) roll'd towards the west end of the church, where it enter'd the belfry: it there broke a very large stone of the floor near the west door into several pieces, and threw a great part of the stone from its place, and stopp'd the church clock, which was near it: from thence ascending the steeple, it divided the great iron rod or spindle of about 50 feet long (composed of several joints fixed into square sockets, and convey'd from the clock for turning the hand of a dial, plac'd in the south front of the steeple) out of their respective sockets, which were much forced and rent: broke and twisted the iron wire of the chimes and clock from the belfry to the bell-chamber (being about eighty feet high) in a most extraordinary manner; some of the wire being much burnt, and in sundry places, melted into little grains. It then enter'd the bell-chamber, threw a large bell off the brass it hung upon; forced the said brass out of the beam, broke off part of the gudgeon, and shatter'd the said beam and frame of the bell: made several breaches in the east and west, but mostly south walls and quoins, split the arch of the south window, which was over the said bell, and drove



out some large stones near it. It then passed out of the steeple about that place, and struck off part of the arch on the outside, together with a large piece of the stone window-frame adjoining; then ascended about four feet higher (which was near the top of the steeple) and beat off a large piece of an old carved Gothic stone head, without injuring the leaden pipe, which came out of its mouth.

Though many people happen'd to be in different parts of the church, yet providentially no one received any hurt.

The belfry was so full of smoke, attended with a strong sulphureous smell, that they, who went thither immediately after the accident, were almost suffocated; and they apprehended, that some part of the church or steeple was on fire, and a watch was kept all night in the church for fear of what might happen.

N. B. The lime and stone were in many places so far affected with the lightning, as to be easily reduced into a powder, by the bare pressure of the finger.

About the same time of the day, two horned cattle in a wood, near two miles south-west of the town of Moulton, were struck dead under a large oak, and the tree itself appeared much scorched.

And in another parish, about the same distance to the south-east of Moulton, and likewise at the same time, three sheep which were lying together in a field were likewise kill'd; the ground under them having two holes made about 2 feet deep each, one of them almost perpendicular, and the other at about a foot distance, more oblique.

About

About 5 or 6 paces farther from the place where the sheep were lying, towards the north-west the ground was much torn up as if plough'd, and an oblique hole made of about three feet deep.

The breadths of the different holes were from six to three inches.

LIII. *A Letter from Mr. James Dodson to Mr. John Robertson, F. R. S. concerning an Improvement of the Bills of Mortality.*

S I R,

January 13, 1752.

*Read Jan. 16, 1752.* **A**S there has lately been a scheme proposed for amending the form of the bills of mortality of London, in a pamphlet called *Observations on the past growth and present state of London*, by Mr. Corbyn Morris, the ingenious author of which has enumerated many excellent purposes, to which it may be applied, but has omitted to mention that of giving a greater degree of certainty to the calculations of the values of annuities on lives; a benefit too considerable to be passed by silently: And as your knowledge of that subject will enable you to judge of what is fit to be done, in order to obtain so desirable an advantage; I beg leave to trouble you with my thoughts concerning a farther regulation of those bills, which, I presume, may be conducive thereto.

The present possessors of intailed estates are, in common law, justly called tenants for life. Marriage-

riage-settlements, generally, convey the reversion of a considerable part of the bridegroom's estate to the bride, for her natural life after his decease; to which two things all the freehold estates in these kingdoms are liable: and if to these be added the great number of copyholds, determinable on lives; the great quantities of church, college, and other lands, leased on lives, and the estates possessed by ecclesiastical persons of all degrees; we shall find, that the values of the possessions and reversions, of much the greatest part, of the real estates in these kingdoms, will, one way or other, depend on the value of lives. Likewise the incomes annexed to all places, civil and military, all pensions, and most charitable donations, are annuities for life. The interest or dividend's of many personalities in the stocks have been, by the wills of their possessors, render'd of the same kind; besides which, there are some annuities on lives, which have been granted by the government, and have parliamentary security for their payment; and others, that have been granted by parishes, in consequence of acts of parliament made for that purpose.

After this summary view of the extensive property, that is vested in annuities on lives, it would be very easy to name a great variety of circumstances, in which the computations of the values of one, two, or more lives, will become necessary to those persons, who do not chuse to have their property determined by customs, which seem to have been established merely for want of good methods of calculation. But I know to whom I am addressing,  
and

and shall therefore forbear to exemplify on a subject, with which you are so well acquainted.

The advantages attending the determination of those things, by calculation, rather than by custom, being therefore considered as evident, it may seem strange, that, notwithstanding many of these tenures have subsisted from the very origin of private property in these kingdoms, yet we do not meet with so much as an attempt towards computing their values, till that of the late justly celebrated Dr. Halley, by the assistance of the bills of mortality of Breslaw in Silesia, which was soon followed by Mr. De Moivre's truly admirable hypothesis, that the decrements of life may be esteemed nearly equal, after a certain age.

It has been the opinion of some authors, that, since his hypothesis was originally derived from the Breslaw observations, it cannot be near so well adapted to the inhabitants of these kingdoms, as what has been derived from the bills of mortality of London. But this argument doth not, as I conceive, appear to be conclusive; first,

Because those bills, as hitherto kept, are not well adapted to answer this purpose.

Secondly, Because the manner, in which the inhabitants of London, and those of most of the country towns and villages, live, their occupations, diet, and diversions, nay the very air they breathe, are as different, as those of London, and Breslaw, can possibly be; and, consequently, so must the times of their dissolution. All which has been, with a great deal of clearness, evinced by the gentleman above quoted.

Thirdly,

Thirdly, because those persons, who suppose, that Mr. De Moivre's hypothesis has its foundation, peculiarly, in the Breslaw observations, are greatly mistaken: for, having lately been endeavouring to discover some farther helps to the speedy valuation of lives, I have found, that, on the contrary, if the London observations had been then in Mr. De Moivre's hands, he might, as justly, have derived his hypothesis therefrom; which will appear from his own words, in the preface to his treatise of *Annuities on Lives*, compared with the London observations.

“ Two or three years after the publication of the  
 “ first edition of my *Doctrine of Chances* (says that  
 “ excellent mathematician) I took the subject into  
 “ consideration; and consulting Dr. Halley's table of  
 “ observations, I found, that the decrements of life,  
 “ for considerable intervals of time, were in arithmetic  
 “ progression: for instance, out of 646 persons of 12  
 “ years of age, there remain 640, after one year:  
 “ 634, after two years; 628, 622, 616, 610, 604,  
 “ 598, 592, 586, after 3, 4, 5, 6, 7, 8, 9, 10  
 “ years respectively; the common difference of those  
 “ numbers being 6. Examining afterwards other  
 “ cases, I found, that the decrements of life, for  
 “ several years, were still in arithmetic progression,  
 “ which may be observed from the age of 54 to the  
 “ age of 71, where the difference, for 17 years to-  
 “ gether, is constantly 10. After having tho-  
 “ roughly examined the tables of observations, and  
 “ discover'd that property of the decrements of life,  
 “ I was inclined to compose a table of the values of  
 “ annuities on lives, by keeping close to the tables  
 “ of observation; which would have been done  
 “ with

“ with ease, by taking, in the whole extent of life,  
 “ several intervals, whether equal or unequal. How-  
 “ ever, before I undertook the task, I tried what  
 “ would be the result of supposing those decre-  
 “ ments uniform from the age of twelve; being  
 “ satisfied, that the excesses on one side would be  
 “ nearly compensated by the defects on the other :  
 “ then, comparing my calculation, with that of Dr.  
 “ Halley, I found the conclusion so very different,  
 “ that I thought it superfluous to join together several  
 “ different rules, in order to compose a single one.”

Now the same thing, which Mr. De Moivre mentions above, happens in the table of the London observations; *viz.* out of 510 persons, of 12 years of age, there remain 504, after one year; 498, after two years; 492, 486, 480, 474, 468, 462, after 3, 4, 5, 6, 7, and 8 years respectively; the common difference being 6; and the like happens in other instances, to be met with in the London observations, as published by different authors. Add to this, that, having calculated the value of an annuity on a life of 10 years of age, by both tables, and also by the hypothesis, I find it to turn out thus,

	Years Purchase.
By the Breslaw tables of observations	17,7237
By supposing the decrements of life equal	16,8814
By the London tables of observations	16,3907

From which there seems to be some reason to conclude, that the hypothesis (as it gives an answer less than the Breslaw, and greater than the London observations) may be the best method of the three; And it is farther remarkable, that the result, by the

hypothesis, is nearer to that by the London, than to that by the Breslaw observations.

However, if the argument for using the London observations has any force at all, the computation of the value of each person's life must be made from observations, drawn from the bills of mortality, kept at the place of his or her residence : and therefore it is, that I at present trouble you, in order to contribute, as much as I can, to there being a sufficient number of good bills of mortality.

There seems to be an objection, both to the hypothesis, and to the observations ; for it is well known, that the fair sex (especially at two periods of their life) are obnoxious to fatal disorders, not incident to the other sex, nor distinguished in the present bills of mortality ; and, consequently, neither the tables of observations, nor the hypothesis (which is derived from them) will render the calculations of the values of lives sufficiently certain ; unless there be a periodical distinction of sexes in those bills : as it would probably appear, if such a distinction had been introduced, that there is a wide difference between the values of a male and female life of the same age.

But there will be a great inconvenience, in rejecting the hypothesis, which none of these gentlemen have remedied ; I mean the prolix and laborious computation hitherto directed for the finding the values of lives from tables of observations : whereas, by the hypothesis (as its author justly observes) more can be concluded in a quarter of an hour, than can be performed in a quarter of a year, by any method, which the others have demonstrated. Whence it may be presumed, that the hypothesis will continue

to be used, until better methods are substituted in the place of those derived from it.

When the bills of mortality, digested into a proper form, shall have been kept a convenient time in every city or considerable town, and also in every hundred, or other proper division, of the country (and this I should be glad to see done) then, and not till then, the hypothesis may be tried by the facts, that will appear from the bills, and be confirmed or rejected accordingly.

Indeed (for my own part) I am almost persuaded, from what has been above remarked, that the hypothesis will, in general, appear to be the nearer the truth, the more those bills of mortality shall be in number, and the correcter they are kept. I shall proceed, therefore, to mention those alterations, which, I think, may be of advantage, in the form of the bills of mortality, in every part of these kingdoms, over and above those mentioned by Mr. Morris, in the before-quoted pamphlet.

1. That there be a distinction made, upon the face of the bills of mortality, between the persons who were born in the place where such bills were kept, and those that were not. This will be effected with a very little trouble, if the searchers of each parish be instructed to ask the question of the friends of the deceased, and annex the answer to their report. This precaution will facilitate many of the good purposes proposed by Mr. Morris; and, in particular, with regard to the fixing the values of lives, it will enable the persons, who shall apply the bills to calculation, to draw their conclusions only from the lives, that were both begun, and ended, in or



near the same place ; the want of the possibility of doing which is the principal objection to the London bills, as hitherto kept.

2. That there be a distinction, with regard both to age and disease, made upon the face of the bills, between the sexes ; and that one case be added to the list of diseases ; *viz.* complaints peculiarly incident to the female sex. This will not only solve the difficulty above started, but also answer many purposes in political arithmetic, as well as to the sagacious physician.

3. That a farther division be made in time ; for whereas Mr. Morris's scheme exhibits no age between 40 and 50, I would propose, that the numbers dying between 40 and 45, and between 45 and 50, should be particularized in the bills ; the design of this being to fix the periods, that are fatal to the fair sex, with more certainty.

These alterations, together with those proposed by Mr. Morris, being made, the yearly bill of mortality, for London, will appear as in the specimen annexed.

Now, Sir, if you shall, upon considering what I have offered, be of opinion, that the above regulations are worthy notice, your approbation will be a sufficient justification of my desire, that they may be made more public, in such manner as you shall think fit. I am, Sir,

Your most humble servant,

James Dodson.



# A GENERAL BILL of the WEDDINGS, BIRTHS, and BU of London (distinguishing a

		One Month old, and under.		From One to Three Months.		Three Months to One Year.		One Two
		Natives.	Others.	Natives.	Others.	Natives.	Others.	Native.
Abortive.	Males							
	Females							
Ague.	Males							
	Females							
Apoplexy, Palsy, Cramp, and suddenly.	Males							
	Females							
Asthma and Phthisic.	Males							
	Females							
Chin-cough, Whooping-cough, and Worms.	Males							
	Females							
Child-bed, Miscarriages, and Feminine Disorders.	Females							
Colick, Gripes, Twisting of the Bowels, Flux, Vomiting, Looseness, Bloody-Flux, and Bleeding.	Males							
	Females							
Consumption, Jaundice, Diabetes, and Falling Sickness.	Males							
	Females							
Convulsions.	Males							
	Females							
Dropsy, Tympany, Stoppage in the Stomach, Rising of the Lights, and Swelling.	Males							
	Females							
King's-Evil, Itch, Leprosy, Scurvy, St. Anony's Fire, Head, Impostume, Sores, Ulcers, Cancer, Fistula, and Mortifications.	Males							
	Females							
French.	Males							
	Females							
Not so.	Females							
Fever, Calentures, Inflammations, Sore Throat, and Quinsy.	Males							
	Females							
Gout, Rheumatism, and Sciatica.	Males							
	Females							
Hemorrhoids, Strangury, and Stone.	Males							
	Females							
Rickets and Infantile Disorders in the Head, Liver, Spleen,	Males							
	Females							
	Males							
	Females							

Index :

[illegible]

### BIRTHS.

*Years inclusive.*

Beyond 1651  
1651 to 1660  
1661 to 1670  
1671 to 1680  
1681 to 1690  
1691 to 1700  
1701 to 1705  
1706 to 1710  
1711 to 1720  
1721 to 1730

1746  
1747  
1748  
1749  
1750

# Index



LIV. *A Letter from Monsieur Le Cat, M.D. first Surgeon at the Hotel Dieu at Rouen, Royal Professor and Demonstrator of Anatomy and Surgery, Member of the Royal Academy of Surgery at Paris, and of the Academies of Sciences at Paris, London, Madrid, and Rouen, to Dr. Mortimer, Secretary of the Royal Society, concerning the Dissection of a Rupture. Translated from the French by Tho. Stack, M.D. F.R.S.*

S I R,

Rouen, June 1, 1750. N.S.

Read Jan. 23, 1752. **I**T is now about eleven years since I had the honour of sending you an account of an incomplete hernia, the strangulated part of which mortified, and by nature's resources alone suppurated, threw off the gangrened parts, and was converted into a fistula: thro' which fistula, in process of time, the two ends of the gut, that were near the strangulation, passed, and fell into the groin, turning inside out, so that the villous coat was on the outside; which gave me an opportunity of making experiments on the effect of purgatives. This observation, which I barely mention here, is printed in *Phil. Transf.* N. 460, p. 716.

When I sent you those remarks, sir, on the singular hernia of Catherine Guillematre, I had already made some fruitless attempts to cure her, but had not then lost all hopes of success. I imagined, that a long  
use

use of emollient cataplasms might restore suppleness to the intestine *B* (Fig. 2. Plate IV. N° 460. and the figure hereto annexed) which constantly kept out of the belly, and was turned inside out, because it was the portion continuous to the *cæcum*, *colon*, *rectum*, and *anus*, which could be of no use, but much incommoded the patient by this extraordinary situation. But all my trials were of no avail, altho' they were carried so far, as to render this gut quite bloody: its long exposure to the air made it become too thick and hard; and at the same time so robust or insensible, that all these vigorous applications made no bad impression on the rest of the animal œconomy. In fine, Catherine Guillematre quitted our hospital without any other benefit but that of having afforded us an opportunity of instructing ourselves.

From that time I had no news of this woman till the 6th of May of this year 1750; when I was informed, that her body actually lay in our dead ward, and that she died in our hospital of old age and a broken constitution, as much as of any disease.

I was extremely curious to embrace this opportunity of having ocular demonstration of the probable conjecture, which I had made in this woman's lifetime, and a confirmation of my having solved the *ænigma*, arising from this singular hernia.

The annexed figure, which I drew from nature, represents the state of the parts, somewhat less than the natural size. In order perfectly to understand what follows, it will be necessary to have Plate IV. of N° 460, before the eye, together with this drawing.

*Explanation*







*p. 343.*





*Explanation of the Figures.*

- A*, The herniary fistula, which does not appear in the figures of N° 460; because the issue of the two portions of the gut, forced into this place the bottom (or back part) of the gut; which unites these two portions; that is, the part of the bore of the gut opposite to that which was mortified, and fell off in an eschar, by the strangulation and suppuration of this incomplete hernia.
- B*, Part of the *ileum* situated between the strangulation and the *anus*, and consequently continuous to the *cæcum*, *colon*, *rectum*, and therefore useless: it is also the same, that is marked *B* in Fig. 2. Plate IV. N° 460, which I said always continued out, and on which I had made so many unsuccessful trials, in order to reduce it.
- b*, Is the continuation of this useless portion of the *ileum*, which at one end is immediately continuous to the *cæcum d*, and at the other thrusts itself into the thick portion *B*, at the extremity *B* of which it turns up, the villous coat outward. This portion *b* is, as may be observed, become very slender, both by its want of action, and by its situation within the other portion *B*.
- C*, The other portion of the *ileum*, situated between the strangulation and the stomach, marked *A*, N° 460, actually returned into the belly but moving out and in alternately, and performing the office of an *anus*, while the patient was alive.
- c*, The part of intestine, which (after the mortification) remained common to both portions *B*, *C*, the edges

edges *f*, *g*, of which are cicatrised to the edges of the herniary fistula. This drawing affords an ocular demonstration of the solidity of the conjectures made in the observation N. 460.

*D, d*, The *cæcum*, and its vermicular appendix.

*E*, A portion of the *colon*, filled with somewhat like *feces*, but which had no other smell than what is natural to the intestines, without the least mixture of a stercorarious stench. This substance was of the colour of white resin, and of a fat viscid consistence : and it seemed to be formed of lymph, and the intestinal juices thickened by heat.

*F*, A portion of the *colon*, which was empty, and its cavity was about three lines in diameter.

*G*, The continuation of the *ileum*.

I have the honour to be, sir, with the highest esteem,

Your most humble, and

most obedient servant,

Le Cat.

LV. *An Account of Dr. Bohadsch's Treatise, communicated to the Royal Society, intitled, Dissertatio philosophico-medica de utilitate electrificationis in curandis morbis, printed at Prague 1751: extracted and translated from the Latin by Mr. Wm. Watfon, F. R. S.*

Read Jan. 23, 1752. **T**HE treatise, of which I now offer an extract to the Royal Society, was sent hither from my friend and correspondent Professor Bose at Wittemberg, who is always desirous of testifying his zeal and attachment to the Royal Society, by communicating to us whatever he imagines worthy our notice. The author of this treatise, Dr. Bohadsch, is a Bohemian, a very learned and ingenious gentleman, who, while he was in England about two years since, was frequently at our meetings, and was very conversant with, and much esteemed by, many of our body, from whom he received very great civilities. He was more particularly taken notice of by his Grace the late Duke of Richmond, whose loss we yet lament: His Grace did me the honour to recommend him to me, as a gentleman not less remarkable for his great knowledge in various kinds of literature, than for his exemplary modesty: and it is with great pleasure that I lay before you what comes from the hands of one, for whom I have so great an esteem.

This treatise, from its title, promises only an account of the advantages of electrification in medicine: but this is not the whole of which it treats; it exhibits also a series of observations of the effects of electricity upon both solid and fluid bodies, upon animals in a state of health, as well as upon those distemper'd. Of each of these I propose to lay before you some account in the course of this extract.

Our author first takes notice, that electricity, being continued for some hours, lessens the weight of the body electrified. He exemplifies this first on fluid bodies; two equal portions of which, before electrifying, he accurately weighs; and then the difference between these two portions, one of which has been electrified between four and five hours, and the other, though in the same room, not electrified at all, is attributed to the operation of the electric effluvia. His globes, I observe, are rubbed by the hands of an assistant.

Four ounces of river water exposed in a glass vessel of four inches diameter were electrified five hours, and lost in their weight eight grains.

Four ounces of river water, in the same kind of glass, but not electrified, lost in the same time only three grains. The difference then to be attributed to the electricity was five grains. The like quantities of the fluids hereafter mentioned were exposed, as the water was, and the effects were as follow.

	Grains
Oil of olives, by electrifying, lost . . .	o
Vinegar . . . . .	ij.
Water impregnated with nitre . . .	iiij.
New milk . . . . .	iv.
	Urine

	Grain.
Urine . . . . .	vij
Spirit of turpentine . . . . .	vij.
Spirit of wine . . . . .	viiij.
Volatile spirit of fal ammoniac . . . . .	xi.

Four ounces of rain-water were exposed in a tin vessel of four inches in diameter, and electrified as before, and the loss was ten grains.

A like quantity of the same water under the same circumstances, electrifying excepted, lost only three grains. In this instance, the effect to be attributed to the electrifying was seven grains.

He then put to the trial, in a tin vessel instead of a glass one, the several liquors before-mentioned; and except the oil of olives, the water impregnated with nitre, and the milk, the rest lost by electrifying a few grains more of their weight.

He afterwards exposed three ounces and a half of river water in a glass vessel, whose diameter was but an inch, and this lost by a like electrification only two grains. The same quantity of water, under the same circumstances, electrifying excepted, lost in the same time nothing of its weight: so that, in this instance, the effect to be attributed to the electricity was two grains. The various liquors before-mention'd were likewise electrified in a vessel of the like capacity as that containing the last water, and they lost much less by the operation, than when they were exposed under a larger surface. All these liquors, electrified for the space of ten hours, as well in vessels of tin, as of glass well stopped, lost nothing of their weight. From hence our author concludes, 1. That electricity



augments the natural evaporation of liquors, unless those of a viscous kind, as oil of olives, which from their tenacity lose nothing of their weight. 2. That electricity increases the evaporation of liquors in proportion as they are more or less volatile: for volatile spirit of sal ammoniac suffered a greater evaporation, than either spirit of wine or spirit of turpentine. These last lost more than water, and even this lost more than the solution of nitre and the vinegar, as we see by the experiments. 3. That electricity operates most in those vessels, which are most permeable to its effluvia, *viz.* in vessels of metal more than those of glass. 5. That the effects of electrifying are not observed in vessels closely stopped.

He afterwards put to the trial several substances of a more solid form. A pear weighing four ounces and a half, electrified five hours, lost of its weight 6 grains. A pear of the same kind, not electrified, lost nothing: so that the difference arising from electrification was 6 grains. He then subjected other substances to this trial, and the effects were as follow.

	Grains.
A piece of dry oak lost	o
A bunch of keys	o
Two new-laid eggs	ij
A piece of new crum of bread	ij
— raw beef	ij
— salt beef	iv
— sponge lightly moisten'd	vi
A bunch of grapes	vij

From

From these experiments our author observes, that the electricity diminishes the weight of solid bodies, if these are impregnated with humours liable to evaporate: for the dry wood, metals, and other bodies, which seem to have no fluids, lose nothing of their weight; and therefore it is only upon the fluids in them that the electricity operates.

Our author then exhibits some experiments made by persons of credit, in order to discover, whether or no electricity would accelerate the growth of plants; and from several trials found that it did. There then follows a series of experiments, which prove, that electricity augments the transpiration of animals. These experiments were made upon puppies, pigeons, yellowhammers, and chaffinches; and the effects of those electrified, compared with those of the same kind, which were not, evince, that electricity does increase the transpiration of animals. Our author here has annexed several curious tables, comparing the loss of weight of the animals, while electrifying, to what they lose in the same time without electrifying. Whoever therefore is desirous of perusing them, must consult the work itself.

Dr. Bohadsch proceeds to give us a theory of those distempers, in which electricity seems to have the greatest effects. He confines himself however more particularly to the *hemiphigia*; of which distemper he gives us the history, corresponding with what we find in the best medical writers. He likewise gives us the usual method of cure, and shews, that the attempts of relieving this malady by electricity, nearly square intentionally with the remedies most celebrated in practice. That the electrical sparks  
and

and commotion produce the same effect, though in a more powerful manner, as warm sulphureous baths, frictions, sinapisms, stinging with nettles, &c. generally made use of in the cure of this distemper. This reasoning does very well in theory; but I should have been glad to have seen it justified by practice, and his own observations. But instead of these, our author contents himself with giving us over again the lying stories of Pivati: to which he has added the four cases published some time since, and transmitted to the Royal Society, as well as to myself, by Professor Sauvages, of Montpellier. These cases indeed do credit to electricity, but we want more of them.

Our author finishes this dissertation, by deducing several conclusions from what he has premised, and these are as follow.

- I. That electricity may be advantageously applied to medicinal purposes.
- II. That it augments the natural transpiration of animals.
- III. That this acceleration of transpiration in men is through the exhaling capillary vessels, and not through the subcutaneous glands.
- IV. That the nervous fluid may be called the electrical fluid.
- V. That the nerves subservient to sensation are not different from those subservient to motion.
- VI. That the immediate cause of the *hemiplegia* is the impermeability of the nervous fluid through the nerves.
- VII. That of all other distempers the *hemiplegia* seems most properly the object of electricity.

VIII.

- VIII. That it may be of use also in intermitting fevers.
- IX. That a palsy in the left side of the body is owing to the right side of the brain, and *vice versa*.
- X. That anger, the parent of numerous evils, is sometimes useful to paralytics.
- XI. That as long as the paralytic limbs are rigid, it is an argument, that the burfal ligaments of the joints, and the sheaths of the tendons, are deficient in the fluid, adapted by nature for their lubrication.
- XII. That every species of palsy does not arise from the nerves being either obstructed, or compressed.

In concluding this account, I cannot help observing, that, contrary to his usual modesty, our author has been guilty of a slight plagiarism in this work; as, without quoting his author, he has translated from the French into Latin the tables above-mention'd, as well as his experiments, proving that electricity forwards vegetation, from our worthy brother the Abbé Nollet's treatise, intitl'd, *Recherches sur les causes particulieres des phenomenons electriques*. See Nollet pag. 358 to 380. Dr. Bohadsch has only alter'd the date 1747 to 1750. But it is to be remember'd, that these accounts were calculated for the meridian of Prague, and not for those of London and Paris.

LVI. *An Account of an horizontal Top, invented by Mr. Serfon, by Mr. James Short, F. R. S.*

Read Feb. 6, 1752. **T**HE horizontal top, the invention of Mr. Serfon, who was unfortunately lost in his Majesty's ship the Victory, is pretty well known. This ingenious person found, that, when this top is set a-going in the proper way, its upper side, which is polished, about two minutes after it was set up, moved in such a manner, as to give a true horizontal plane; and that this plane was not at all disturbed by any motion or inclination you give the box, in which it is placed, and therefore might be proper to be used aboard a ship; by which means seamen might be enabled to take the altitude of the sun or stars, in order to find their latitude, even tho' they cannot see the horizon in thick hazy weather.

Some gentlemen of my acquaintance were of opinion, that the air had some share of the cause of this horizontality. I therefore applied to Mr. Smeaton, who has the best air-pump I ever saw, all of his own invention and construction. The pump being at this time in the house of Mr. William Watson, who had desired the use of it for some electrical experiments, we went thither; and having set the top a-going, we put a receiver over it, and immediately exhausted the air.

By repeated trials it had been found, that the top, when set a-going in the open air, played or spun  
during

during the space of 35 minutes of time, from the instant of its being set up till it had lost the circular motion : but we found, that in the exhausted receiver it played or spun during the space of two hours 16 minutes \* ; and therefore, that the air has no share at all of the cause of its horizontality, and that the air is a great impediment to its motion.

London, Feb. 6,  
1752.

Ja. Short.

LVII. *Observations made in going up the Pic of Teneriffe, by Dr. Thomas Heberden, and communicated by William Heberden, M. D. F. R. S.*

Read Feb. 6,  
1752.

AT two of the clock in the afternoon we set out from the villa or town of Orotava, about 6 leagues distant from the Pic of Teneriffe. The weather was cloudy ; and before we had travell'd quite a league, we found ourselves surrounded by a very thick mist or fog, which lasted about a league : all which time we travell'd among gardens and woods of pine-trees, after which we came to an open country ; the soil very dry ; here and there a single pine-tree, and some few Spanish broom-plants ; some loose large stones, of the bigness of a butt ; others, which seem'd to have been burned, and are supposed to be cast out from the vulcano of the Pic. The sky very clear, and the thick mist, which we had passed thro', now seem'd a sea of ash-colour'd clouds

Y y

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\* Preserving a perfect horizontality for the space of  $\frac{1}{4}$  of an hour.

clouds below us. Having travell'd two leagues on this soil, we arrived at eight o' clock in the evening at the Falda del Pico, or foot of the Pic. Here we were obliged to leave our horses; the road, by reason of its steepness and loose sandy soil, being impassable to them. At half a league's distance we baited under some large rocks, called La Estancia de los Ingleses, or the English baiting-place, being first used as such by some of our countrymen in ascending the Pic. Here we tarried all night, making fires to temper the air, which we found very cold. When the morning drew near, we proceeded on our journey, ascending for a quarter of a league the same soil (but more steep and loose) till we arrived at some large rocks of mal-payfes (or stone burnt by a vulcano); amongst which, as the ground was more firm, we walk'd with less trouble, or rather climbed, being frequently obliged to make use of our hands to help us forward.

Having gone about a quarter of a league in this manner, we arrived at the famous cave of Teyde. It is surrounded on all sides (or rather buried) with large mal-payfes, or vulcanian rocks, between which you discover the entrance about six feet high, and four feet wide. The cave seems to be about fifteen feet wide at the entrance; the extremity we could not discover. From its entrance to the surface of the water, which covers the bottom, seems to be about twelve or fourteen feet. The top and sides of the cave are of smooth stone. The bottom is cover'd with ice or snow; above which is a body of water about half a yard deep. This cave is the grand reservoir of snow of the island, whence they are supplied,

plied, when their common reservoirs, which they prepare for cooling their liquors, fail them.

At somewhat more than a quarter of a league's distance from the cave, we came to a plain of sand; from the middle of which arises a yellowish pyramid of sand or cinders, which the inhabitants call *La Pericosa*, and we *The Sugar-loaf*; around the basis whereof perspire vapours incessantly. The Sugar-loaf is about an eighth part of a league to the top, which is very difficult of ascent, occasioned by the loose soil, and steepness of the road. About eight o' clock in the morning we gained the summit or caldera. It is about twelve or fifteen feet deep: the sides, sloping down to the bottom, form a concavity, or *crater*, resembling a truncated cone, with its base uppermost. The *crater* seems nearly circular; its diameter about forty fathom. The ground is very hot; and from near twenty *spiracula*, as from so many chimneys, you perceive a smoke or vapour of a strong sulphureous smell. The whole soil seems mix'd or powder'd with brimstone, which forms a beautiful colour'd surface.

There is one of the rocks, which forms a sort of vault or nich; against which the vapour condensing produces what the inhabitants call *Azufre de Gota*, or *Drop-Brimstone*. The nich, against which the vapour is condens'd, is of a greenish colour, sparkling with yellow like gold. The same colour you perceive on almost all the stones thereabout. A small part of the Sugar-loaf is white like lime; and another lesser part there is, whose internal substance seems a sort of red clay, and whose superficies is cover'd with a salt.



In the middle of one of the rocks was a hole, about two fingers breadth in diameter, whence proceeded a noise like a great body of liquor boiling very strongly; and one of the company applying his hand to the *spiraculum* at about a quarter of a yard distance, was burnt for his curiosity.

This Sugar-loaf is covered with snow the greatest part of the year. The snow was lying on it from October 1742 to June 1743.

The different accounts of various authors concerning the height of this famous Pic would have incited one less inquisitive than I am to satisfy his curiosity, by examining the real altitude thereof: for which end, between three and four o' clock in the afternoon of a very serene day, when not a cloud appeared, either on the summit, or in the whole atmosphere, (to prevent any accidental refraction) having pitched on a plain along the sea-side for my horizontal stand, and measuring trigonometrically a base sufficiently corresponding to the angles with the greatest accuracy, I observed the height to be 2566 fathoms.

Two subsequent observations by myself, as well as two antecedent ones some years before by John Crosse Esq; the British consul, served only to confirm my opinion of the justness of this observation.

Tho' the body of the mountain is covered with clouds, the Pic is generally seen above them quite clear; tho' sometimes the contrary happens; the whole body of the mountain without a cloud, and only the summit of the Pic covered with a thick white cloud, as with a cap. This is often observed in the finest weather; and the Spaniards, on this occasion, say, *El Pico tiene su sombrero puesto;*  
(i. e.)

(i. e.) ‘The Pic has put his little hat on;’ and look on it as a certain sign of rain.

During the 6 or 7 years, that I lived in the villa of Or tava, as I had a continual sight of the Pic, I have several times observed the above phænomenon, and do not remember one instance, in which the prediction of rain failed.

LVIII. *Observations of the Weather in Madeira, made by Dr. Thomas Heberden, and communicated by William Heberden, M. D. F. R. S.*

Read Febr. 6, 1752. **T**HE thermometrical observations are made with Fahrenheit’s thermometer, and the calculations deduced from two observations daily; at seven o’clock in the morning, and at three in the afternoon. The same method of calculation is to be understood of the barometer. The rain fell thro’ a funnel 15 inches in diameter.

The Lesté, Levant, or hot winds, are very troublesome. The remedy is, to keep ourselves within-doors. October 1749, comparing 2 of Fahrenheit’s thermometers together, one of them exposed on the north side of my house to the open air, the other within-doors, the difference was as follows:

	Hour	Therm. within-doors.	Therm. exposed to the air.
Lesté, Oct. 20.	10	73	81
	12	76	82
	4	77	77
MADEIRA			

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MADEIRA, Anno 1749.

Barometer

Thermometer

	Mean Height	Greatest Height	Least Height	M. H.	G. H.	L. H.
March	29.81	30.2	29.8	64.66	70	61
April	30.075	30.2	29.8	60.7	68	64
May	29.55	30.1	29.6	66.53	69	65
June	30.017	30.15	29.75	68.75	72	66
July	30.027	30.1	29.95	74.58	75	72
August	30.013	30.1	29.95	75.07	77	74
September	30.054	30.15	29.85	76.53	78	72
October	29.841	30.	29.7	72.2	77	68 LW
Novemb.	29.68	30.	29.55	68.6	73	67
December	29.675	29.9	29.4	64.9	68	62

Anno 1750.

January	29.195	29.8	29.4	64.	68	62
February	29.692	29.75	29.5	63.8	67	61 LW
March	29.12	29.65	29.3	66.5	.	51 LW
April	29.285	29.4	29.1	66.45	68	65
May	29.775	29.9	29.5	66.25	68	65
June	29.875	30.1	29.5	69.06	72	6
July	29.887	29.95	29.8	73.	75	71
August	29.386	30.1	29.75	75.4	78	72
September	29.915	30.05	29.7	74.93	77	72
October	29.797	29.9	29.5	73.87	77	70
Novemb.	29.875	30.05	29.55	70.825	76	67
December	29.843	30.2	29.5	66 27	74	64

N. B. LW signifies a Levant or hot wind, in Feb. and March.

An

An Account of the Quantity of Rain, which has fallen in the Island of MADEIRA.

Anno	1747	1748	1749	1750
	Inch. Decs.	Inch. Decs.	Inch. Decs.	Inch. Decs.
January	20 .525	8. 600	2 .097	7 .150
February	.485	10. 958	1 .203	1 .771
March	4. 339	5. 241	.932	1 .123
April	.528	. 722	.777	.039
May	.353		5 .290	1 .087
June	1 .321	. 420	.113	.226
July	.200			.176
August	.018	2 .700		.003
September	.540	.810	.855	1 .682
October	.010	3 .303	1 .512	6 .601
November	5 .181	2 .654	3 .059	5 .611
December	7 351	1 .500	6 .527	1 .882
	140 .851	37 .508	22 .365	27 .351

The years 1749 and 1750, were such dry years, that the corn was destroy'd, and the fruit-trees suffer'd much, particularly the peach-trees, the fruit either falling to the ground, whilst green, or, if it remained longer on the tree, being full of white worms.

IX. *Extract of a Letter from Mr. Willem Van Hazen to Mr. Philip Miller, F. R. S. concerning the Quantity of Rain, which fell at Leyden in the Year 1751.*

Read Feb. 20, 1752. **D**URING the course of the last year 1751, it rain'd no less than 163 days ; and the quantity of rain, which fell, was 41 inches.

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LX. *An Account of a double Child, communicated to the Right Honourable the Lord Willoughby, of Parham, F. R. S. by Thomas Percival Esquire.*

My Lord,

Read Feb. 20, 1752. **A**BOUT three weeks ago was born a remarkable child at Hebus near Middleton. I presume somebody or other will send the Royal Society an account of it ; but, lest that should not be soon done, be pleased to accept the inclosed, given me by a neighbouring surgeon. I have not myself seen it, being confined to my room with the gout, but am well assured it is exact, having shewn it to many, who have, and who all agree it to be right. am, my Lord,

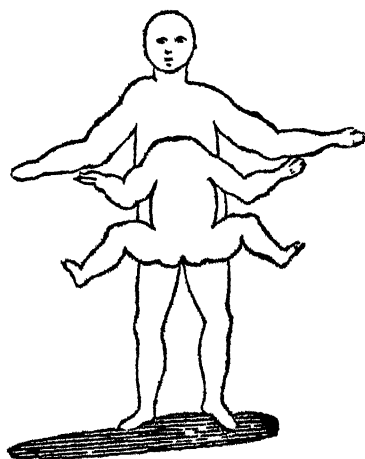
Your Lordship's most devoted,

Feb. 10, 1752.

Tho. Percival.

*The*

*The Portraiture of an uncommon Child, born January 1752, of the Wife of Richard Tong, of Hebus near Middleton, 5 Miles from Manchester in the County of Lancaster.*



The child, or children, if they may be so called, are both females. The one is a perfect healthy-looking fine girl. The imperfect one adheres to the perfect one by the *cartilago ensiformis*, by a cartilaginous substance 4 inches in circumference. The body seems to be of a soft fleshy substance of very little regularity: it has no head, nor neck, nor any respiration: out of the upper parts of its body come out two short arms. On the right, which is the longer, are 4 fingers, but no thumb on the left, which is very short, its hand is very deficient, and upon it only two fingers. The thighs, legs, and feet, are the most perfect, tho' the legs have only one bone in them. It has no *vertebræ* of the back or

loins. The *os sacrum*, as well as the *os pubi*, imperfectly ossified. All its joints are very rigid and stiff. It has no *anus*, but passes off its water in the natural way. Its *sternum* is very imperfect; and it has no *clavicula*. It seems insensible of pain, not removing its arms or legs, if laid in an uneasy posture.

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LXI. *An Account of the Phenomena of Electricity in vacuo, with some Observations thereupon, by Mr. Wm. Watson, F. R. S.*

To the Royal Society.

Gentlemen,

Read Feb. 20, 1752. **I**N a paper I had the honour to lay before you in January 1747, which was the last I communicated to you of my own upon the subject of electricity, and which has been since publish'd in the *Philos. Transf.*\*, I acquainted you, that I intended upon some future occasion to lay before you a series of experiments in electricity made *in vacuo*; from a comparison of which with those already made in open air it did appear, that our atmosphere, when dry, was the agent, by which, with the assistance of other electrics *per se*, we were enabled to accumulate electricity in and upon non-electrics; that is, to communicate to them a greater quantity of electricity than these bodies naturally have. That, upon the removal

\* Numb. 485, p. 120.

removal of the air, the electricity did pervade the *vacuum* to a considerable distance, and did manifest its effects upon any non-electric substances, which did terminate that *vacuum*; and that by these means, originally-electric bodies, even in their most perfect state, put on the appearance of non-electrics, by becoming themselves the conductors of electricity.

I had not so long delayed the illustration of these opinions by the experiments, which put me in possession of them, but that I was not only diverted therefrom by very various avocations, but desirous of giving them a still greater degree of perfection, in order to place the above deductions beyond all controversy. The executing the apparatus necessary hereto was not easily surmounted: I unsuccessfully tried several artificers, who were not able to arrive at the nicety, which I thought necessary in the construction of my instruments. Animated however by a late very honourable occasion, and assisted by Mr. Smcaton in the completing my apparatus, the event fully answered what I proposed; although from the experiments I had made before the communication of the above accounts, I was fully convinced of their truth. I had other opinions indeed, which did still require a further degree of demonstration.

To make these experiments succeed, two things were more particularly required; first, that the inside of the glasses made use of should be perfectly dry; and therefore it was necessary, that their internal surface should be exposed to the wet leathers, usually employed in pneumatic experiments, as little as might possibly be; otherwise, the vapours, arising therefrom



in exhausting, defeated the intent by conducting the electricity, and thereby preventing its accumulation. Secondly, the more complete the *vacuum* was, *cæteris paribus*, the more considerable were the effects: and here I should not do justice to real merit, were I silent in regard to Mr. Smeaton. This gentleman with a genius truly mechanical, which enables him to give to such philosophical instruments, as he executes, a degree of perfection, scarce to be found elsewhere; this gentleman, I say, has constructed an air-pump, by which we are empower'd to make Boyle's *vacuum*, much more perfect than heretofore. By a well-conducted experiment, which admits of no doubt as to its truth, I have seen by this pump the air rarefied to a thousand times its natural state; whereas commonly we seldom arrive at above one hundred and fifty. As the promotion of the mechanic arts is a considerable object of our excellent institution, if this gentleman could be prevailed upon to communicate to the Royal Society that particular construction of his air-pump, which enables it to execute so much more than those commonly in use, it would not fail to be an acceptable present: but to return:

The experiments treated of in this paper must be considered to have been made in this *vacuum*. The electrical machine, with its prime conductor, need here no particular description; but that of the glass, in which the *vacuum* was made, should be more minutely considered. It consisted of a glass tube nearly three feet in length, and of almost three inches in diameter. A ring of brass, exactly fitting this tube, was cemented to both its extremities, into each  
of

of which was screwed a hollow brass cap, nearly of an hemispherical figure. Into the top of one of these caps was adapted a brass box of oiled leathers, through which was admitted a slender brass rod of a length sufficient to reach within eight inches of the other extremity of the tube. Into the top of the other brass cap was fastened a brass rod, like the former, only of eight inches in length. Thus the extremity of one of these brass rods might at pleasure, without letting in the air, be made to touch the other; and for the better observing what difference in effect would arise from an increase of surface, a small brass circular plate was made to screw into each of these extremities. As the sight of this instrument will convey to you at once a more clear idea than the most accurate description, I take the liberty of laying it before you.

The intent of being able to bring the extremities of these rods near together, and to separate them again to what distance you pleased, was, that it might without difficulty be determined, whether, and to what distance, the electrical fluid would manifest itself *in vacuo*, further than in air of the same density with the external.

The tube then thus fitted, and made dry both within and without, was placed in a cylinder of brass, of about two inches long, and of a diameter just sufficient to admit the brass cap before-mentioned; and round the rim of this brass cylinder, to prevent the ingress of air, was adapted a narrow piece of wet leather. These being placed upon the plate of the air-pump, which stood upon cakes of wax, a piece of wire passed from the prime conductor to the long  
brass

brass rod, at the other extremity of the tube, and by these means, upon setting the electrical machine in motion, the long brass rod in the tube was electrified. When the brass plate at the bottom of this rod was placed near, or even at the distance of two inches from the plate of the other rod, the brushes of electrical fire were seen passing from the periphery of the upper plate to that of the lower, and every part of the air-pump snapped upon the touch of any one standing upon the floor, and gave the other usual signs of the accumulation of electricity. But, as these plates were made to recede from each other, this effect grew less and less; so that, when they were removed five or six inches from each other, no snaps could be drawn from the air-pump; as the dissipation of the electric fluid was now as easy from every part of the prime conductor, as from the upper brass plate in the tube: but it is to be noted, that this distance is different, as from the weather or other circumstances the electricity is more or less strong.

Upon exhausting this tube, and electrifying as before the air-pump still standing upon cakes of wax, the electrical fire was not only seen to pass from one plate to the other at the distance of 5 inches, but the same effect ensued at the greatest distance, to which in the tube the brass plates could be drawn. Being therefore desirous to see a farther effect, and to avail myself of the whole length of this tube, I took from the inside of it the short brass rod, to which the lower brass plate was fixed, and fasten'd this plate at the very bottom of the tube into the cap. The consequence was, that the electricity, meeting with scarce any resistance, passed from the

top to the bottom of the tube, and electrified the air-pump as before: and it was a most delightful spectacle, when the room was darkened, to see the electricity in its passage; to be able to observe, not, as in the open air, its brushes or pencils of rays an inch or two in length, but here the coruscations were of the whole length of the tube between the plates; that is to say, thirty-two inches, and of a bright silver hue. These did not immediately diverge as in the open air, but frequently, from a base apparently flat, divided themselves into less and less ramifications, and resembled very much the most lively coruscations of the *aurora borealis*.

At other times, when the tube has been exhausted in the most perfect manner, the electricity has been seen to pass between the brass plates in one continued stream of the same dimensions throughout its whole length; and this, with a subsequent observation, seems to demonstrate, that the cause of that very powerful repulsion of the particles of electrical fire one to the other, which we see in open air, is more owing to the resistance of the air than to any natural tendency of the electricity itself; as we observe, that the brushes thereof from blunt bodies, when the electricity is strong, diverge so much, as to form, when seen in the dark, an almost spherical figure. This figure seems therefore to arise from the electricity's endeavouring to insinuate itself between the particles of air. The figure, that an elastic fluid of less density must form, when let loose, and equably compressed by one more dense and more elastic, must necessarily approach to that of a sphere.

Upon

Upon admitting a very small quantity of air into the tube, these phænomena disappeared; not so much from the small quantity of air admitted, as from the vapours, which insinuated themselves therewith. These lined the sides of the glass, and conducted the electricity imperceptibly from one end of the tube to the other. And to illustrate farther, that the vapours, and not the air, in the small quantity admitted, occasion'd this total disappearing of these phænomena; upon experiment they have been visible, though in a less perfect degree, when a much larger quantity of air was omitted to be exhausted from the tube.

These experiments seem to evince, that however great the *vacuum* could be made, the electrical co-ruscations would pervade it through its whole length.

From hence it appears, that our atmosphere, when dry, is the agent, by which we are enabled to accumulate electricity upon non-electrics; as in the experiment before us, upon the removal of it, the electricity passed off into the floor through a *vacuum*, of the greatest length we have hitherto been able to make, became visible in this *vacuum*, and manifested itself by its effects upon the air-pump, being the non-electric substance, which terminated that *vacuum*: whereas, when the air is not taken away, the dissipation of the electricity is from every part of the prime conductor. We see here also, contrary to what we have found hitherto, that an originally-electric body, *viz.* a dry glass tube, puts on the appearance of a non-electric, by becoming itself the conductor of electricity, that is, by its keeping out the air, and suffering the electricity to pervade the *vacuum*.

How

How much soever the *vacuum*, here made use of, may exceed that, which is usually arrived at, it is far from being a perfect one; and to make one more so, except that of Torricelli, which cannot without difficulty be applied to the present purpose, is not very easy. But it appears from the already mentioned experiments, as well as from a subsequent one, that the *vacuum*, which we are masters of, does not transmit the electricity so perfectly as metals and water; as we are able to draw snaps from the prime conductor, an argument of some degree of accumulation, while the electricity is passing through the *vacuum*. This never happens, when metals, standing upon the ground, touch the prime conductor. As we observe therefore, that the coruscations diverge more or less, in proportion as there is more or less air left in the tube, this effect may arise even from the small quantity of air still remaining undischarged.

I was desirous of knowing, for the farther illustration of my propositions, whether the experiment of Leyden could be made through the *vacuum*. For this purpose I made the before-mention'd exhausted tube part of the circuit, so necessary to this experiment. What this circuit is, I have in my former communications so often and so clearly exemplified, that it would be needless to repeat it here. You know in this experiment it is likewise absolutely necessary, that the whole quantity, or nearly so, of the accumulated electricity should be discharged in the same instant of time. Accordingly, upon making the experiment, at the instant of the explosion, you saw a mass of very bright embodied fire jump from one of the brass plates in the tube to the other: but this did not take place, when one of the plates was

A a a

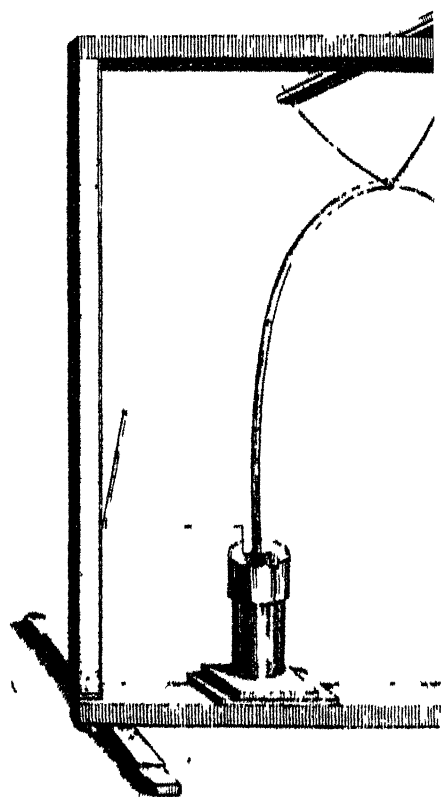
farther

farther distant from the other than ten inches. If the distance was greater, the fire then began to diverge, and lose part of its force; and this force diminished in proportion to its divergency, which was nearly as the distance of the two plates.

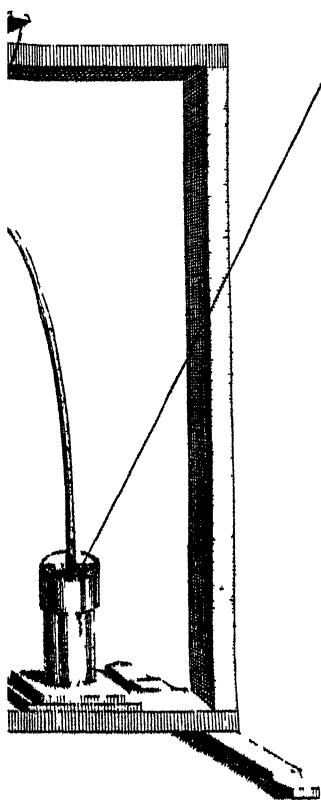
The difficulty however of applying the Torricellian *vacuum* to these experiments has been happily got over by the right honourable the Lord Charles Cavendish, our worthy Vice-president. This noble lord, who to a very complete knowledge of the sciences joins that of the arts, and whose zeal for the promotion of true philosophy is exceeded by none, has applied it in the following manner, and his lordship has had the goodness to put his apparatus into my hands. This apparatus consisted of a cylindrical glass tube of about three tenths of an inch in diameter, and of seven feet and half in length, bent somewhat like a parabola in such a manner, that thirty inches of each of its extremities were nearly straight, and parallel to each other, from which an arch sprung, which was likewise of thirty inches \*. This tube was carefully fill'd with mercury; and each of its extremities being put into its basin of mercury, so much of the mercury ran out, until, as in common barometrical tubes, it was in equilibrio with the atmosphere. Each of the basins containing the mercury was of wood, and was supported by a cylindrical glass of about four inches in diameter, and six inches in length; and these glasses were fasten'd to the bottom of a square wooden frame, so contriv'd, as that to its top was suspended by silk lines the tube filled with







p. 370.



J. A. G. J. A. G.



with mercury before-mention'd; so that the whole of this apparatus without inconvenience might be moved together. The Torricellian *vacuum* then occupied a space of about thirty inches. In making the experiment, when the room was darkened, a wire from the prime conductor of the common electrical machine communicated with one of the basons of mercury, and any non-electric touching the other bason, while the machine was in motion, the electricity pervaded the *vacuum* in a continued arch of lambent flame, and as far as the eye could follow it, without the least divergency.

That the electricity was not furnished from the glasses employed in these operations, nor from the circumambient air, I have heretofore, in my communications to you upon this subject, endeavoured to evince. I have shewn, that electricity is the effect of a very subtil and elastic fluid, occupying all bodies in contact with the terraqueous globe; and that every-where, in its natural state, it is of the same degree of density; and that glass and other bodies, which we denominate electrics *per se*, have the power, by certain known operations, of taking this fluid from one body, and conveying it to another, in a quantity sufficient to be obvious to all our senses: and that, under certain circumstances, it was possible to render the electricity in some bodies more rare than it naturally is, and, by communicating this to other bodies, to give them an additional quantity, and make their electricity more dense: and that these bodies will thus continue until their natural quantity is restored to each; that is, by those, which have lost part of theirs, acquiring what they have lost;

and by those, to which more has been communicated, parting with their additional quantity. Both one and the other of these is, from the elasticity of the electric matter, attempted to be done from the nearest non-electric; and when the air is moist, this is soon accomplished, by the circumambient vapours, which here may be considered as preventing in a very great degree our attempts to insulate non-electric bodies. But these matters I have copiously treated of in my former communications upon this subject\*: this short recapitulation however I thought necessary, for the more easy illustrating what I propose to subjoin; and it is upon these principles that we are able to account for the circulation of electricity described in the *Philosophical Transactions*, Vol. XLIV. p. 740.

If therefore the before-mention'd principles are true, and if the electricity is not furnished by the globe in its rotation, nor by the air, it ought to be visible in the *vacuum* of the before-described glass tube, in its ingress to the frame of the electrifying machine, if this machine, and the man who turns the wheel thereof, are supported by electrics *per se*, and if, during this operation, the electricity, as fast as furnished, is taken off by a bystander, or otherwise, from the prime conductor; as under these circumstances the *vacuum* is the only passage open to its progress, and from its elasticity the electricity should protrude itself through it. And from experiment this is the case; for, upon a piece of wire being connected with the end of the long brass rod,

\* See *Phil. Transf.* Vol. XLV. pag. 95 & seq.

rod, or with the brass cap at the upper extremity of that tube and the other end of the wire fastened to any part of the frame of the electrifying machine, and this last put in motion, the electrical coruscations are seen to pass as before from one of the brass plates contained in the tube to the other; and to continue, unless the air insinuates itself, as long as the machine is in motion. If, under these circumstances, the hand of a person standing upon the floor is brought near the sides of the glass, the coruscations will direct themselves that way in great variety of forms, extremely curious to behold. But here, as in the former experiment, our *vacuum* did not conduct so perfectly as metals or water; as a person, standing upon the floor, and applying his finger to the upper brass cap of the tube, receives a smart stroke: and this I conceive to arise, from the electricity of this brass being so much more rarefied, or attenuated, than that of the body of the man, applying his finger.

This experiment should be made in the middle of a large room, and the machine, and man turning it, should be raised from the floor at least a foot: otherwise the effects desired will be diminished by the electricity being in part furnished by the floor to the machine.

To what is here laid down it may be objected, that the electrical coruscations in the last experiment proceed, not from the floor of the room, as I have conjectured, but from the electricity being, from the globe in motion, diffused at the same time upon the prime conductor, as well as all over the machine, and which in the tube becomes visible in its passage

to the floor. But it is to be remember'd in this experiment, that no electricity is perceptible either *in vacuo*, or upon any part of the machine, as above-mentioned, unless at the same time the prime conductor is made use of; for, without that, there will be no diminution of the density of the electricity in the machine, as the quantity taken from the cushion by the globe in its rotation is returned upon it again the next revolution, the cushion being the first non-electric, which offers itself: but this I have have consider'd at large, as may be seen in the *Philosophical Transactions* \*. This experiment therefore, in which the electricity is seen, without any preternatural force, pushing itself on through the *vacuum* by its own elasticity, in order to maintain the equilibrium in the machine, which had lost part of its natural quantity of electricity by the present operation; this experiment, I say, I do not scruple to consider as an *experimentum crucis* of the truth of the doctrines here laid down; to wit, not only that the electricity is furnish'd by those bodies, hitherto called non-electrics, and not by the electrics *per se* ||; but

\* Vol. XLV. p. 96.

|| Since the communication of this paper to the Royal Society in February 1752, *viz.* in the succeeding summer, the truth of this doctrine is put out of all doubt by the discovery made in France, in consequence of Mr. Franklin's hypothesis, of being able, by a proper apparatus, to collect the electricity from the atmosphere during a thunder-storm, and to apply it to the usual experiments, which demonstrates, that the matter of thunder and lightning and that of electricity are one and the same. That the electricity did not proceed from the glass, or other electrics *per se*, as they had been usually called, I first discover'd in the year 1746.  
See

but also, that we are able to add to, or take from, that quantity of electricity, naturally adherent to bodies.

By what denomination shall we call this extraordinary power? From its effects in these operations, shall we call it electricity? From its being a principle neither generated nor destroyed; from its being every-where and always present, and in readiness to shew itself in its effects though latent and unobserved, till by some process it is produced into action, and rendered visible; from its penetrating the densest and hardest bodies, and its uniting itself to them; and from its immense velocity; shall we, with Theophrastus, Boerhaave, Niewentyt, s'Gravesande, and other philosophers, call it elementary fire? Or shall we, from its containing the substance of light and fire, and from the extreme smallness of its parts, as passing through most bodies we are acquainted with, denominate it, with Homberg and the chemists, the chemical sulphureous principle, which, according to the doctrines of these gentlemen, is universally diffused? We need not indeed be very solicitous in relation to its denomination: certain it is, that the power we are now treating about is, besides others, possessed of the properties before mentioned, and  
cannot

See *Phil. Trans.* Vol. XLIV. p. 713.—749, and explained further Vol. XLV. p. 95, *et seq.* and though the electric matter may be taken from the atmosphere during a storm of thunder, or even when it is only charged with what are usually called thunder-clouds, that is, when the atmosphere is replete with heterogeneous phlogistic matter, yet it must not be considered as coming from pure dry air, which, as I before mentioned, I conceive to contain in its natural state scarce any of the electric matter, and is the agent, by which we are enabled to communicate electricity to other bodies.



cannot but be of very great moment in the system of the universe.

I am, Gentlemen, with all possible respect,

London, Feb. 12, 1752. Your most obedient humble servant,

W. Watfon.

*LXII. A Letter from Dr. Bevis to Dr. De Castro, F. R. S. containing Extracts of Father Augustin Hallerstein's astronomical Observations made at Pekin in 1744 and 1747.*

Read March 5, 1752. **I** AM much obliged to you, Sir, for furthering F. Aug. Hallerstein's letter to me. It informs me, that the instrument I wrote the description and use of, was arrived safe at Pekin. According to that missionary's request, I have carefully looked over the observations he sent to Dr. Sanchez at Paris, to be communicated to the Royal Society through your hands. They are comparisons of all the planets with known fix'd stars taken in the Jesuit's College at Pekin, in 1746 and 1747, with a well-adjusted pendulum-clock, and a micrometer; and appear to me to have been done with judgment and accuracy

accuracy; so as, in my humble opinion, to merit the Royal Society's consideration. I am,

Dear Sir,

Feb. 18, 1751.

Your obliged and

most obedient servant,

J. Bevis.

Observationes Lunæ 1747.

**J**AN. 1, mane, mox a media nocte, observata occultatio reguli ( $\alpha$   $\beta$ ) aluna ut sequitur.

46	o luna alta circ. $59^{\circ}$ capta ejus diameter	31	48
55	29 dist. $\alpha$ $\beta$ a limbo lucid. propiore lunæ	48	37
23	35	37	35
35	49	51	12
49	53 distantia ejusdem ab eodem	17	20
o	10	23	17
16	44]	16	44

3  $\alpha$   $\beta$  immerfit post limbum lunæ lucidum in linea recta ducta per Grimaldi mediam, et Copernici limbum superiorem (situ recto) observata emerfito tubo 10 ped.

5 15 51  $\alpha$   $\beta$  emerfit de sub lunæ limb. obscuro in linea recta per limb. superiorem Grimaldi, et inferiorem Copernici (situ erecto) observata emerfito una simul tubis 10 et 5 ped.

- 25 19 dist.  $\alpha$   $\Omega$  a limb. remotiore lucid. lunæ 35 27  
 30 12 dist. ejusdem ab eodem 37 37  
 39 56  $\alpha$   $\Omega$  in horario  
 42 23 lunæ limb. remotior lucidus in horario, erat-  
 que  $\alpha$   $\Omega$  borealior limbo austrino lunæ 34 3  
 6 15 o capta rursus diameter lunæ 31 40 alta circ.  
 43°.

Jan. 28. vesp. comparata luna cum stella  $\rho$   $\Omega$ , quæ  
 a luna occultata fuerat, sed immersio quidem videri  
 non potuit, luna post tectum templi adhucdum  
 latente, itaque

- 9 29 57 emerfit stella de sub parte obscura lunæ, tum  
 vero  
 34 42  $\rho$   $\Omega$  in horario  
 36 40 limb. lucid. lunæ remotior ortivus in horar.  
 eratque  $\rho$  borealior limbo austrino remotiore  
 lunæ 29 29  
 porro diameter lunæ per oblivionem non  
 adnotata est.

Feb. 25, vesp. comparata luna cum Regulo five  $\alpha$   
 $\Omega$ , quem illa quidem texerat, sed neque immersio  
 visa neque emissio, luna post tectum templi la-  
 tente; simul autem ac apparuit,

- 6 42 54  $\alpha$   $\Omega$  in horario  
 44 o margo occiduus lunæ in horario  
 45 56 macula Aristarchus in horar. australiori  
 stellæ  $\alpha$  o 12  


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 3 2 differentia temporar. stellæ  $\alpha$  ab Aristarcho  
 7 26 capta diameter lunæ 32 4  
 42 39  $\alpha$   $\Omega$  in horar. accurate in eodem parallelo  
 cum limbo lunæ, dum is postea circa hora-  
 rium esset  
 47 28 Aristarch. in horar. austral. stella  $\alpha$   $\Omega$  13 6  
 Cum

Cum ergo  $7^h 47' 28''$  Aristarch. esset australior stella  $\alpha \Omega$   $13' 6''$ , margo autem boreus lunæ, dum hujus centrum circa horarium esset, eundem præcisè parallelum decurreret, quem decurrerat stella  $\alpha \Omega$  liquet Aristarchum margine boreo lunæ australiorem fuisse itidem  $13' 6''$ . Erat autem idem Aristarchus orientior margine occiduo lunæ  $1' 56''$  penduli: hinc facile erit appulsam centri lunæ ad horarium eruere, habita jam diametro lunæ  $32' 4''$ .

Tempora quod attinet harum operationum, corrigenda sunt singula, demendo  $1' 38''$  penduli. Totidem enim anticipasse sequenti meridie compertum est.

## Observationes astronomicæ habitæ Pekini in Collegio S. J.

### Observatio $\zeta$ 1746.

	$h$	$'$	$''$		$o$	$'$	$''$
Nov. 28 mane	5	20	49	$\zeta$ occidentalior in $m$	0	3	0
				borealior		40	2
29	5	44	0	borealior in $m$		37	36
				distabat ab eadem		37	54

### Observat. $\gamma$ 1746.

Jul. 13 vesp.	8	0	0	$\gamma$ occident. $\omega$ Ophiuc.	0	12	32
				borealior		13	33
	8	15	0	distans ab $\omega$		16	43
Aug. 25	8	0	0	$\gamma$ orientior $\omega$	0	1	30
				borealior		0	45
				distans ab $\omega$		1	57
26	7	18	0	$\gamma$ orientior $\omega$	0	6	1
				borealior		0	0
				dist.		5	28
				B b b 2			Aug.

Aug. 28	8	15	0	4	occidentalior $\omega$	0	47	22
					borealior		15	17
29	8	15	0	4	occidentalior $\omega$	0	48	53
					borealior		15	18
4					pene stationarius diff.		48	20
Aug. 4	9	0	0	4	occidentalior $\omega$	0	49	38
					borealior		14	3
					diff.		49	30
5	8	0	0	4	occidentalior $\omega$	0	49	8
					borealior		13	39
					diff.		48	10

Observationes 8 1746.

Sept. 29	mane	5	0	0	♂	orientalior	$\alpha$	21	48
						borealior		45	53
						diff.		50	9
Nov. 14	mane	4	59	56	♂	orientalior	$\beta$	49	38
						borealior		35	19
25		4	8	13	♂	occidentalior	$\gamma$	41	7
						borealior		33	42
						diff.		53	15
Dec. 20		3	12	12	♂	occidental.	$\theta$	20	3
						borealior		6	9

## Observationes 2.

Nov. 5 vesp.	6	5	0	2	occidentalior stella	
					quadam ignota	0 35 35
					aeque borealior	7 18
6	6	16	18		ignota heri occiden-	
					talior quam $\phi$ $\pi$	6 54 53
					aeque borealior	7 18
					Nov.	



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	"	"	"	"	"	"	"
Jun. 24	8	21	21	♂ occidental.	74 <sup>m</sup>	o	18 55
				australior			29 3
25	8	47	13	♂ occidentalior	74 <sup>m</sup>	o	18 48
				australior			29 16
				♂ itaque directus			
Jul. 23	8	2	33	♂ orientalisior	74 <sup>m</sup>	o	20 25
				et australior			53 1

Observationes ♀ 1747.

Feb. 3 mane	6	6	48	♀ occidentalior	28	♀ o	31 5
				et australior			31 30
				dist.			32 35
13	5	39	38	♀ occidentalior	ν	♀ o	17 47
				australior			0 58
28	5	37	26	♀ orientalisior	o	♀ o	1 23
				australior			47 5

Observationes ♂ 1747.

Jan. 5 mane	3	20	8	♂ occidentalior in	♠	o	24 4
				borealisior			14 28
6	3	13	17	♂ occidentalior in	♠	o	4 31
				borealisior			3 31
Feb. 15	5	43	46	♂ orientalisior	μ	♠ o	6 1
				australior			11 33
				cum max. lat. boreal.			
Apr. 30 mane	3	51	1	♂ orientalisior	α	♠ o	10 32
				borealisior			11 22
♂ ⊙ ♂ vesper	9	9	5	♂ occidentalior	α	♠ o	5 16
				borealisior			20 30
Maii 1 mane	3	55	0	♂ occidentalior	α	♠ o	12 17
				borealisior			21 44
13 vesp.	8	16	24	♂ occidentalior	μ	♠ 4	28 44

Maii

	2	1	11		2	1	11
Maii 13	♂	prope	♂	auftralior		38	4
16	7	44	22	♂ occidentalis $\mu$	5	27	54
				auftralior		26	34
Jun. 10 vesp.	8	21	17	♂ occid. $\lambda$ $\mu$	2	19	53
				auftralior		45	16
Oct. 27 vesp.	6	7	14	♂ occidentalis $\chi$ $\tau$	4	39	45
				borealis		12	18
Dec. 31 vesp.	6	7	13	♂ occident. $\sigma$ $\omega$	0	54	8
				auftralior		6	33

Observationes ♀ 1747.

Jan. 8 vesp.	5	41	20	♀ orientalis $\beta$ $\nu$	1	4	41
				auftralior		23	32
Mar. 5 mane	5	43	38	♀ occident. $\beta$ $\nu$	0	13	32
				auftralior		22	51

Observationes ♀ 1747.

Jan. 16 mane	6	4	53	♀ occidentalis $\xi$ $\tau$	7	43	16
				borealis		24	13
19	5	58	18	♀ occidentalis $\xi$ $\tau$	5	49	13
				auftralior		2	38
23	6	3	5	♀ occidentalis $\xi$ $\tau$	2	8	36
				auftralior		33	34
24	6	7	3	♀ occidentalis $\xi$ $\tau$	1	4	41
				auftralior		40	7
25	5	55	18	♀ occidentalis $\xi$ $\tau$	0	2	45
				auftralior		45	24
		57	18	dist. a $\xi$ $\tau$		45	39
26	6	18	37	♀ orientalis $\xi$ $\tau$	1	16	12
				auftralior		50	2

Jan.



	h	m	s		h	m	s
Jan. 26	6	26	35 <sup>8</sup>	occidentalior w <sup>z</sup>	0	26	49
				et australior		10	28
	31	56		distans ab eadem		26	52

Congressus planetarum observati 1747.

Jan. 13 mane	2	51	54 <sup>h</sup>	in horario			
		52	43 <sup>8</sup>	in horar. austral.	47	2	
Dec. 6 vesp.	5	34	34 <sup>8</sup>	immersit totaliter			
				sub limb. obsc. $\mathfrak{p}$			
				dist. a cornu bor.	23	28	
				tum lunæ diam.	32	53	
	6	46	2 <sup>8</sup>	emerfit de sub $\epsilon$ dist.	29	24	
				a cornu boreo ejusdem.			

LXIII. *Extracts of several Letters of Mor-  
dach Mackenzie, M. D. concerning the  
Plague at Constantinople.*

*Dr. Clephane, F. R. S. to the Rev. Mr. Birch,  
Secr. R. S.*

S I R, Golden-Square, Feb. 25, 1752.

Read March 5,  
1752.

**B**EFORE I transcribe my friend Dr.  
Mackenzie's letters relating to the  
late plague at Constantinople, it may not perhaps be  
improper to mention a few particulars concerning the  
plague in general, as I find them scatter'd up and  
down his former letters to me on that subject.

In

In a letter dated March 24, 1749, he observes, that, in his time, the plague, whether at Constantinople, Smyrna, or any other part of the Levant, has been mostly sporadic, seldom epidemical. That therefore the articles in our news-papers, which so often mention the plague raging violently, are almost always false.

At Constantinople, and all over the East, people, he says, shun the plague, and the infected, as much as we do; and every body, physicians as well as others, who have been with the sick, or in places infected, are all obliged to perform forty days quarantine.

The Armenians and priests are the only people, who attend them; and they only to give them necessaries at a distance, or to perform the last functions of the church; and this the priest is obliged to do by his religion.

The European plagues are much more violent than the eastern; those being really the Thucydidian, which sweep all away; while these are only gentle corrections to put us in mind of mortality.

The doctor, in another letter, finds fault with the method used in England to prevent infection by shipping; "for, to what purpose (says he) keep ships  
" in Sandgate-Creek for weeks, and even months,  
" without landing and serening the goods? I hope  
" you will allow, there is little to be feared from the  
" bodics of men, who get in good health from Smyrna  
" to England, which voyage is seldom performed in  
" less than 7 or 8 weeks; which I presume will be  
" thought too long for infection to remain in the  
" blood without producing some effect. Wherefore,  
" as all the danger is from the goods or cargo, greater

“ care ought to be taken of this, and less of the men.  
 “ Your nation differs much from Italy or Marseilles,  
 “ where a ship may, and often does, arrive in eight  
 “ days ; for which reason, tho’ it be necessary to look  
 “ after the men, as well as the goods, still however  
 “ they make a great distinction. You make none.”

It is observable, that from the beginning to the *status* or *acme* of the disease, they almost all die : afterwards its violence begins to abate, and about the end of the season most people recover.

The symptoms of the distemper are chiefly ; irregular fits of heat and cold ; shiverings ; violent head-ach, and reachings, for the first three or four days ; great anxiety about the *præcordia*, &c. both before and after the eruptions ; a wild staring countenance ; sweats for the most part about the head and breast only, at the same time the extremities cold ; a dry parched yellow-furred tongue. The more violent those symptoms are, the greater the danger ; *et e contra*. Some are delirious, and raving ; others to a great degree stupid and dull : both these are fatal appearances. Some die in 5 or 6 days ; some outlive 20 days, and then die : some walk the streets for many days, and afterwards die. Bleeding at the nose is reckoned a salutary sign.

A swelling in the throat is a common symptom ; for which if you bleed, it proves almost always fatal : for it is so far from abating this symptom, that after it a greater difficulty of breathing ensues, and the patient seldom survives it above 3 or 4 hours.

The physical writers are divided as to the expediency of bleeding in the plague, some contending for it warmly, others as warmly condemning it. The  
 doctor

Doctor distinguishes between the different stages of the distemper. and says, that as in the beginning, during the ebullition, bleeding may be of some service, so when the disease is advanced, and especially after the eruptions, it will prove fatal, as well as purging, or any other violent evacuation.

A moderate diaphoresis ought always to be kept up.

To the buboes, parotides, &c. they commonly apply a roasted fig with some white sug r powder'd : and this they reckon the best suppurative.

They do not open the tumors, but leave them to break of themselves.

They give the sick cold water to drink, and order the cool regimen quite thro' the distemper.

*Copy of Dr. Mackenzie's first Letter concerning the late Plague at Constantinople.*

Constantinople, July 23, 1751.

“ WE have at present the most violent plague,  
 “ that has been at Constantinople in my  
 “ time, by all reports; for I know nothing of  
 “ it, as I live at the mouth of the Black Sea  
 “ for security; but, as I am informed, few or  
 “ none escape; which shews, that the malignity  
 “ is not yet come to its state. They are all taken  
 “ the same way, with a shivering, and vomiting, a  
 “ violent head-ach, thirst and fever, of which they  
 “ die the third or fourth day, rather in a stupor  
 “ than a delirium; and such, as have the misfor-  
 “ tune to be near the infected person, are taken in  
 “ seven or eight days, tho' there are already many  
 “ instances to the contrary. I presume many die  
 “ of

“ of other diseases, which are all laid to the account  
 “ of the plague; for there is no other mentioned at  
 “ present, and there is a very great consternation  
 “ among the people. The Greeks and Armenians  
 “ suffer most, next to them the Jews. The Turks  
 “ suffer less in proportion than other nations. The  
 “ Franks have hitherto escaped, excepting one Jesuit  
 “ priest, who waited on the Christian slaves of the  
 “ grand signor’s bagnio, and died three days ago.”

*Dr. Mackenzie to Dr. Clephane, F. R. S.*

Dear Sir, Constantinople, Nov. 23. 1751.

Read March 5, 1752. **I** RECEIVED yours of the 9 of September on the 23 of October last; and had it arrived 24 hours sooner, I had been very expeditious in answering: but as a courier goes to Vienna from Constantinople but once in a month, the 24 hours your letter came short, make almost a month’s difference.

You are pleased to ask me, if we can account from any apparent causes for the present violence of the plague? To which I answer, that, during the twenty long years I have lived in this country, here and at Smyrna, there has scarcely been a year, excepting three, in which the plague did not threaten more or less; and in all that interval I observed no other difference in the seasons, than that the winters might begin more early, and continue somewhat longer, and with greater rigour; tho’, by my thermometers, this difference never exceeded 5 or 6 degrees; which is no great difference here, where  
 the

the south and north winds make a difference of 15 to 20 degrees in 24 hours: so that I can't see any other apparent cause of the virulency of the disease this year, besides the occasion of greater communication. In the months of February, March, April, and May last, the distemper was so strong at Cairo, as appears by letters from the English consul there, that no doors were open'd for three months. In the mean time there arriv'd here in May last four ships loaden with Cairo goods; which goods and men being landed, spread the infection over all the city at once, after which, one convey'd it to another by contact.

The only apparent cause of the virulency in this case is, four ships arrivng from Cairo, instead of one or two, at the same time; and if you please, you may add to this some little difference of the seasons, mention'd in my letter to Dr. Mead, and a greater quantity of cucumbers, melons, and fruit, than usual, upon which the poorer sort of people feed.

However I don't believe the number of the dead any-ways equal to common report, for the reasons following:

The Turks have no bills of mortality, but they reckon, that in and about Constantinople there are consumed daily 20,000 killows of flour. Every killow is reckon'd to weigh 20 oques, and every oque is equal to 400 drachms, and 160 drachms thought sufficient for a person for 24 hours, or one complete day, taking men, women, and children together. Wherefore one killow makes bread enough for 50 persons *per* day; but the consumption of bread in the months of July, August, and September, was 3000 killows short: from which it is concluded, that

$3000 \times 50 = 150,000$  must have died of the plague, without making any allowance for the great number of people, that run away to Prusa, Nicomedia, Adrianople, the islands, and such as must have died of other diseases in three months in a populous city of a million of souls, by the calculation of 20,000 killows *per* day.

The sickness began very violent, which struck a panic in all ranks and degrees of people, causing many to remove their quarters; and even the better sort of Turks themselves used greater precaution than usual; by which means there were but two of their great men died: 'tis said, four of the grand signor's ladies died; but this wants confirmation. Thus far I can assure you, that in the village where we lived, there died only sixty persons of the plague; and in the year 1740, which made no noise, there died in the same village of it 49 persons. The French ambassador's palace next door to us in the village was infected; because five of his people went at midnight to a bawdy-house, where the father Demetry, the mother and daughter, at the same time had the plague, and died of it afterwards all three; so that two of his excellency's servants were infected by them, one of whom died, and the other recover'd, and is still living, after taking a vomit, some doses of the bark mix'd with snake-root and Venice-treacle, by my advice.

Next I must observe to you, that there are two vulgar errors, with regard to the plague, establish'd in this country. They say, that a plague which begins early, ends soon; which is false; for, in the year 1735, the plague began at Smyrna the

15 of February (by means of a vessel, which convey'd it to Candia, as was said) pretty hot, so that all the houses in Frank-street were shut up in February ; and it continued till the latter end of November.

Another vulgar error is, that the heat kills the plague at Smyrna, and the cold at Constantinople ; which is very true with regard to Constantinople, but very false with regard to Smyrna : for proof look back to the year 1735, when the vigour of the malady shew'd itself most in the months of June and July, tho' so very hot, that some people were said to die of the heat in going from the town to the villages near it : so that it is very certain the heat does not kill the plague at Smyrna, as is generally thought and said.

I am somewhat surpris'd to find there should be a general quarantine order'd in Holland, where there has been none since I have been in Turkey ; and more particularly, since there is not the least appearance, or even suspicion, of sickness at any other scale in Turkey, excepting that of Constantinople ; where, by the by, there does not appear a Dutch ship once in three years.

*Dr. Mackenzie to Dr. Mead, F. R. S.*

S I R,

Read March 19, 1752. **T**HIS is the only summer since I have been in Turkey that I can say we have been without any plague. The air was very temperate, no heavy rains, high winds at N. E. from which point our Etesian winds blow, commonly called



called *milhem* in the Turkish language. Fruits have not been so plenty, or of such a good quality as usual : few fevers of the intermittent kind, but not so regular as usual in their symptoms ; for they were seldom attended with any head-ach, the tongue not much charg'd, and the urine seldom made any sediment of the lateritious kind ; and if they were not taken in time, a yellow jaundice came upon them the sixth or seventh day ; and in the beginning of the fever, the patient seldom vomited bile as usual, but rather a pituitous matter.

I have the honour of being,

S I R,

Constantinople, Oct. 29, 1750. Your most obedient, and

most obliged humble servant,

Mordach Mackenzie.

*Dr. Mackenzie to Dr. Mead.*

S I R,

Read March 19, 1752. **T**HE last, which I had the honour of writing you, accompanying some medals and intaglio's, went by the Thames in October 1750. I have sent by the Bosphorus, Capt. Kennard, a small box, with full direction, containing 50 medals, four intaglia's, one Basilidian amulet, or *προφυλακτικήων*, with *ΣΟΛΟΜΟΝ ΣΩΖΩΝ* cut upon it. He has

has likewise on board a stone, with an inscription, and three figures, *viz.* a mother, and two sons, of whom she takes leave at a funeral repast. The figures of the two sons are somewhat damag'd; but the mother, with a palla covering her head and body, the chair without a back, or rather a stool with a cushion upon the seat, and the three-footed table, at which they sit, are very complete, and well preserved, as likewise the inscription.

We have had last summer the most violent plague, that has been in this country (as it is thought) for the twenty years, which I have lived in it. Some say, that 150,000 souls have died of it in five months; but it is impossible to determine the number, as the Turks keep no registers of such as die of this or any other malady.

The winter began last year very early in November. About the beginning of January the small-pox was very frequent, but not mortal; being for the most part of the distinct kind. It continued to the latter end of March 1751, when malignant fevers began, and continued till the middle of May, when four ships arrived from Cairo with the plague on board. They no sooner landed their goods and men, than it began to spread among the inhabitants, and got to a great pitch by the 10 of June, and extended more and more daily, till the middle of August; when, after a deluge of rain, thunder, and lightning, it was much abated; but it recover'd its strength again about the beginning of September, which it retain'd till the middle of October, when, after some snow, and cold weather, it intirely cess'd, and we are now under no apprehension from it for this winter. 'Tis true, some  
D d d accidents

accidents may happen in houses, which were once infected, and not well purified, all winter long: but these are so rare, that they deserve little or no attention.

I remember to have had the honour of writing to you my sentiments of this distemper some years ago; and from all the observation I could make in the interval, I have no reason to change my opinion, *viz.* that it is brought from Cairo commonly; and that when once a house or ship is infected, it is very difficult to eradicate the *animalcula*, *semina*, *effluvia*, *miasmata*, or whatever name is proper for the reliques or remains of it, which getting once into a *nidus*, lodge there, condens'd by the cold during the winter, and when rarefied by a certain degree of heat, they act upon bodies, which have a disposition, as women and children mostly, and so spread by contact only, without communicating any malignancy to the ambient air. Otherwise very few could escape; whereas we found this last time, and upon all such occasions, that whoever kept their doors shut, run no risque, even if the plague were in the next house; and the contact was easily trac'd in all the accidents, which happen'd among the Franks. Comte Castellane had, for three years running, persons attack'd in the same room, in the months of July and August, notwithstanding all possible precaution us'd in cleansing the room, and even white-washing it. At last, by my own advice to his excellency, grounded upon the above theory, he built a slight counter-wall; since which there has been no accident in that room, now five years ago.

I could give so many such examples, as *delassare valeant Fabium*.

The

The patients were this year sick at stomach, and troubled with vomiting and nausea's for three or four days after they were infected, and before the eruption of the buboes, carbuncles, or tokens ; and in about four days more after the eruptions they died, or shew'd good symptoms of recovery ; such as, the fever, with all its symptoms, decreasing ; the eruptions tending to maturation and suppuration, the nausea ceas'd, and some appetite beginning.

I refer you to my letter to Dr. Clephane, for more upon this subject, and have the honour of being with the most profound respect,

S I R,

Constantinople, Nov. 23,  
1751.

Your most obedient

humble servant,

Mordach Mackenzie.

ΖΩΣΙΜΟΣ ΑΣΠΑΣΙΑΣ ΠΡΟΣΦΙΛΗΣ  
ΧΑΙΡΕ. ΜΑΡΚΟΣ ΑΣΠΑΣΙΑΣ ΠΡΟΣΦΙΛΗΣ  
ΧΑΙΡΕ.

LXIV. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipping Company of Apothecaries for the Year 1751, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. & Soc. Reg. super Præses; by John Wilmer, M. D. clarissim. Societar. Pharmaceut. Londinens. Soc. Hort. Chels. Præfekt. & Prælect. Botan.*

- Read March 15, 1751. 1451 **A** DONIS sylvestris flore luteo, foliis longioribus C. B. 178.
- 1452 Althæa frutescens Lusitanica folio rotundiori undulato. Tourn.
- 1453 Anonis viscosa spinis carens lutea major. C. B. P.
- 1454 Aspalathus frutescens minor angustifolius cort. aureo. Amm.
- 1455 Asteriscus frutescens leucoii foliis, viridibus et splendent. H. E.
- 1456 Bulbocodium crocifolium, flore parvo violaceo T. Cor. 50.
- 1457 Carduus canescens, aculeis flavescentibus munitus. Boerh.
- 1458 Catanance quorundam. Lugd. 1190.
- 1459 Ceratocephalus Virgin. tripteris foliis lævibus, flore luteo radiato. Vaill.
- 1460 Chondrilla viminea. J. B. 2. 1021.
- 1461 Claytonia ————— Linnæi.
- 1462 Corona solis altissima Vosacan dicta. Vaill.

- 1463 *Cruciata Alpina latifolia lævis.* Tourn. 115.
- 1464 *Cyclamen hyeme et vere florens Perficum dictum.* H. Reg. Par.
- 1465 *Cytisus Canariens. microphyllus angust. prorsus incanus.* Pluk. Phyt.
- 1466 *Erigeron foliis inferioribus dentato-laciniat. superioribus integris.* Lin.
- 1467 *Eryngium yuccæ foliis spinis tenellis, hinc inde marginibus appositis.*
- 1468 *Euonymus Novi Belgii, corni feminæ foliis.* Hort. Amst. 86.
- 1469 *Gallium arvense flore cœruleo.* Inst. R. H.
- 1470 *Globularia vulgaris.* Tourn. 466.
- 1471 *Helianthemum saxatile, foliis et caulibus incanis Apennini mont.* Ment.
- 1472 *Jacea foliis cichoraceis villosis altissima, floribus alb. et purp.* T. 444.
- 1473 *Jacobæa Afric. frutescens, foliis incisfis et subtus cineraceis.* Com. Raii.
- 1474 *Iris palust. lutea, seu acorus adulterinus.* J. B. 2. 732. Off. 249.
- 1475 *Lavendula Canariensis, spica multiplici cœrulea.* Pluk. Phyt.
- 1476 *Limonium Ægyptiacum lignosum, halimi folio.* D. Jussieu.
- 1477 *Linaria purpurea major odorata.* C. B. 213.
- 1478 *Malpighia latiore folio subrotundo, fructu majore.* Plum.
- 1479 *Menyanthes palustre et triphyllum.* T. 117. Off. 493.
- 1480 *Napæa Linnæi.*
- 1481 *Nasturtium orient. fol. inferiorib. millefolium, superioribus perfoliatam referentibus.* T. 214.

- 1482 *Ocimum Zeylanicum* perenne frutescens, fol.  
Calaminth. Boerh.
- 1483 *Osteospermum spinis ramosis*. Linn. Hort.  
Cliff.
- 1484 *Oxys bulbosa* African. rotundifol. flor. purp.  
amplis. H. Amst. f. 21.
- 1485 *Polium maritimum supinum* Venetum. C. B.  
221.
- 1486 *Polygala Africana* frutescens, buxi folio, maxi-  
mo flore. Olden.
- 1487 *Punica*, flore pleno majore. T. 636. Offic.  
395.
- 1488 *Sambucus racemosa rubra*. C. B. 456. Off.  
424.
- 1489 *Scabiosa stellata*, fol. non dissecto. Tourn.
- 1490 *Scabiosa stellata minima*. C. B. P.
- 1491 *Scrophularia orient chrysanthemi* fol. fl. mi-  
nimo variegat. T. Cor.
- 1492 *Sicyoides* American. fructu echinato, fol. an-  
gulatis. T 103.
- 1493 *Sida* fol. crenatis, inferioribus cordatis obtusis  
superioribus acuminatis.
- 1494 *Solanum Bahamense* papas floribus. Hort. Elt.
- 1495 *Tanacetum African.* fruticans multiflorum, &c.  
Com. H. Amst.
- 1496 *Thymbra legitima*. Clus. Hist. 358.
- 1497 *Thymbra Sancti Juliani*, five *satureia vera*. Lo-  
bel. Ic. 425.
- 1498 *Verbena urticæ foliis* Canadensis. Cornut.
- 1499 *Virga aurea* fol. latioribus in summis virgis albis  
spicatum dispositis.
- 1500 *Xeranthemum* flore pleno purpureo majore.  
Hort. L.

LXV. *An Account of Dr. Bianchini's Recueil d'experiences faites à Venise sur le medicine electrique; by Mr. William Watson, F. R. S.*

To the Royal Society.

Gentlemen,

Read March 12,  
1752.

**A**BOUT the close of last summer, our worthy member the Abbé N'ollet of Paris transmitted, as a present to the Society, a treatise, intituled, *Recueil d'experiences faites à Venise sur la medicine electrique, par quelques amateurs de physique, publié par M. J. Fortunat Bianchini, docteur et professeur en medicine, et traduit de l' Italien pour servir de correctif à la lettre sur l'electricité medicale.* This treatise, from the misfortune which we labour under from the present bad state of health of our excellent president, to whom it was sent, has not as yet been presented in form to the Society; but as you have already much interested yourselves in investigating the truth of the facts, which occasioned this publication, I take the liberty, from a copy thereof sent me at the same time by my kind friend and correspondent the Abbé Nollet, to lay before you a short account thereof. This indeed may be now thought less necessary, as, since the Abbé's journey to Italy, and our want of success here in our attempts to do the like, every body has consider'd what the Italians printed upon the transmissiion of odours thro' the pores of glass, and upon the subject of medical electricity,



electricity, as too hasty a publication. Mr. Winkler, however from Leipzig sent to the Society, long since these publications, some tubes and globes, which he said had transmitted odours from electrifying. What he conjectured the glasses would do, fell infinitely short of what he first gave out; but even after the most careful trials, and complying with his instructions most scrupulously, we were disappointed in our expectations. I made no doubt therefore, but that the Society would be glad to be informed of what had resulted from the same inquiries elsewhere; and these are the subject of the treatise in question.

The experiments were made by Dr. Bianchini, assisted by several curious and learned men, who frequently assembled for that purpose. These gentlemen, struck with what had been published in relation to medical electricity, and not being able to separate what was true from among such a number of witnesses so directly opposing each other, determined to be guided by the result of their own experiments; and it was by this troublesome, though of all others the most sure way, that they have learned to reject a great number of what had been published as facts and which the love of the marvellous in some, and credulity in others, had contributed to render famous in very distant countries. Having been informed themselves of what was to be depended upon in these matters, they then set about to give others the same information; and this occasioned the present work, where we find decisive experiments upon every question relating to the subject. These have been ingeniously imagined, sensibly conducted, ranged in proper order, robbed of all superfluous reasoning, and  
made

made just in the same manner as those of the academy *del Cimento*, the value of which every one present, I presume, is not now to be apprized of.

The truth of this publication is not to be suspected; it comes from the very place, where medical electricity took its rise; and is not the production of one person, who might be suspected too slightly to have admitted what might tend to favour his own opinions. These are facts consider'd in themselves independently of all application, decisions of the unanimous voice of a number of very sensible men, and in the face of a great number of witnesses, many of them prejudiced to the contrary, and but here forced to be convinced by the evidence of facts.

The gentlemen concerned in conducting these experiments divided them into three classes. The first class contains a series of experiments made with tubes and globes containing odoriferous or other substances, in order to observe, when these were closely stopped, whether the odorous, as well as other effects of the substances included, would pervade the glass. The second class includes experiments made with tubes and globes, which have nothing within them; but the persons electrified hold in their hands, or sometimes place under their naked feet, odoriferous, purging, or even the most poisonous substances, in order to observe, whether the persons electrified in this manner would be sensible of the effects of these substances. The third class gives us a series of experiments different from the two former, in which the substances before-mention'd are mixed with the water, as in making the experiment of Leyden. From these experiments we are to discover, whether from

receiving the shocks from these bottles, the person is sensible of the effects in his body of the substances contained in them.

I should be carried very far, were I to be too particular in my accounts of these experiments: I shall content myself therefore in mentioning to you the bodies employed, and the result therefrom.

These gentlemen tried sulphur powdered, camphor, musk, of all known bodies the most remarkable for its subtilty, volatile sal armoniac, a mixture of turpentine and storax, powder of Benjamin. These odoriferous substances were all severally put to trial in glasses closely stopped, and electrified a reasonable time. After the experiment, there appeared neither in the skin of the persons electrified, nor in the matter they perspired, in their beds, nor about their cloaths, any odours of the substances contained, sufficient to impose upon the most credulous persons.

They next tried in the same manner, whether the usual effects of medicines would be obvious in the persons electrified; and for this purpose quicksilver, gamboge in powder, and liver of antimony, were employed; but, contrary to what had been before published, not the least of their effects were observable. With a like event they tried opium, corrosive sublimate, and cantharides.

The next series of experiments were made by the person electrified holding the drugs, &c. in his hand. The subjects employed here were aloes, scammony, gamboge, opium, and corrosive sublimate. In one of these experiments, a boy of eleven years were electrified with his naked feet  
standing

standing upon cakes of pitch. Under his feet, and upon the pitch, was strewed a large quantity of powder'd scammony, so thick as to prevent his skin from touching the pitch. The scammony stuck to his feet, and his soles were in a manner cover'd with the powder of this drug. During the ensuing night and the next morning, the boy had four copious stools, but without pain or griping.

This effect excited some debates among the society. Some were of opinion, that the purgative power of the drug manifested itself by this new method of administration: others accounted for what had happened, from an alteration in the temperature of the air, which, from hot and serene, had become suddenly cold: some again ascribed it to the washing of the boy's feet, which immediately preceded his electrification; others attributed it to the immoderate quantity of fruit he had eaten. It was moreover insisted upon, that his being acquainted with what might be expected, might even so work upon his imagination as to produce this effect: but as a real matter of fact was the object of the debate, it was thought proper to make a fresh inquiry, without trusting to conjectures. Three days afterwards therefore he was electrified again with a fresh parcel of scammony added to the former, and the operation continued for the same time, and in the same manner, as before; but this produced nothing. No stools follow'd it, as in the former experiment. But to prevent any doubts arising from the above trials, they strongly electrified a healthy youth of about fifteen, with powder'd gamboge under his naked feet, for forty minutes.

During the operation he felt a great heat in his feet and legs, and a considerable quantity of the gum, which the heat had softened, stuck to the soles of his feet; but this person felt no disturbance in his stomach or bowels, and had but one stool in the subsequent four-and-twenty hours. So that, from all these substances applied to the skin, no effects could be attributed to the electrification.

In the third class of experiments the phial was employed, as in making the experiment of Leyden, and was first filled with camphorated spirit of wine. The shock from this was but feeble; whence it was judged, that spirit of wine was not capable of receiving any considerable degree of electricity\*. The phial therefore was emptied of this liquor, and filled with clear water, with which was mixed half a drachm of flowers of Benjamin, and the mouth was closed as before. In making the experiment of Leyden, the stroke then was very severe to the observer, who drew the snap by accident from the wire of the phial. There was no one of the company, who was not desirous of bringing his nose near the electrified glass, in hopes of perceiving the smell of the Benjamin. Some of the company stood upon the resin, and holding their hands either upon the iron bar or the phial, caused themselves to be electrified twenty or thirty minutes; but no one could perceive the least smell of the Benjamin, not even in the hand, that touched the phial.

They

\* The author of this account has consider'd this matter in a paper communicated to the Royal Society some time since. See *Phil. Transf.* Vol. XLV. p. 109.

They afterwards electrified in the same manner a quart of water, in which were dissolved an ounce of gamboge and an ounce and half of resin of jalap. A young man in perfect health grasped the glass containing this mixture between his hands: when he touched the iron bar, he felt a violent shock in his elbows and breast, which was a certain token, that the included mixture was become highly electric. This operation lasted twenty minutes, and yet the young man perceived not the least disturbance in his stomach, nor felt any thing to be attributed to the purgative medicine. It was then tried, whether the same glass would have any effect on persons electrified; for which purpose two young men stood up on the resin, where one staid thirty, and the other forty minutes, holding their hands upon the glass all the time, whereby the electricity was conducted to them, and the sparks drawn from their bodies were very bright: but neither did these perceive in this manner any effects of the medicines.

The last experiment these gentlemen made, was with cantharides powder'd and mix'd with water. This mixture was put into a phial, and three persons held it in their hands successively a considerable time. Neither of the three perceived any difficulty or heat in making water: their urine was neither more nor less in quantity than usual; and they had not the least symptom of any of those complaints, which cantharides never fail to produce, if taken internally, though in very small quantities.

There appears, through the whole course of the experiments contained in the work before us, a great deal of care and accuracy. They were made by  
persons

persons fully acquainted with the manner of employing their apparatus, and many of the experiments were several times repeated.

After what has been done here at London, at Paris, and at Wittemberg, with the like success, these experiments, I presume, cannot, to unprejudiced persons, but be conclusive, that the miraculous accounts from Italy and Leipzig had no foundation in fact; and that no method has yet been discovered, whereby from electricity the powers of medicines could be made to insinuate themselves into the human body.

This conclusion however does not, nor is meant to operate, against the advantages said to be gained by electricity itself. So subtil and so elastic a fluid admitted in a large quantity into our bodies, as, from undoubted experience, it greatly heats the flesh, and quickens the pulse, may, more especially when assisted with the expectation of success in the patient, in particular cases be attended with very great advantages. I am,

Gentlemen,

London, March 10,  
1752.

Your most obedient

humble servant,

W. Watson.

LXVI. *The Case of the Operation of the Empyema, successfully performed by Mr. Joseph Warner, F. R. S. and Surgeon to Guy's Hospital.*

Read March 19, 1752. **T**HE symptoms of an empyema, or of a collection of matter deposited in the cavity of the *thorax*, are sufficiently known to every one of experience in physick and surgery. But the great uncertainty of success attending the evacuation of this matter by operation, has occasion'd surgeons of the greatest eminence to differ about the propriety of the performance of it.

However, as it is notorious, that upon any quantity of extravasated fluid being confined to the *thorax*, the patient not only labours under the most uneasy sensations, but is in very great danger from the injury and oppression, to which the lungs are exposed, in consequence of the disease; and that the success of the operation greatly depends upon the degree of injury communicated to the lungs, and the rest of the contents of the *thorax*; I am inclined to think, for these reasons, as well as from the little danger there is in the performance of the operation, that it is always to be recommended upon the appearance of such symptoms, as indicate such a collection of matter: and whether the following case may be some proof of its propriety under certain circumstances, I beg leave to submit to your consideration.

Thomas



Thomas Hines, aged 27, was admitted into the hospital on the 19 of December last, on account of a pain in his right side, and cough; which he had laboured under for three weeks. He was immediately put under the physicians care; but notwithstanding all proper methods used for his relief, his disorder increased till the 13 of January following, when I was consulted.

Upon inquiry, I found him afflicted with the following symptoms, a quick low pulse, frequent cough, and difficulty of breathing; which last symptom was greatly increased upon lying on his left side, or upon sitting upright. He appeared greatly emaciated, his countenance very pallid, or fallow. Upon farther inquiry, I found the right side of the *thorax* somewhat enlarged; the integuments were visibly thickened, but without the least discoloration, or perceivable fluctuation. However, being persuaded from the foregoing symptoms, that there probably was an extravasated fluid underneath, I advised the operation, which was accordingly done upon the spot, in the following manner:

The patient being conveniently seated, I made an incision of about three inches long, with a knife, betwixt the tenth, and eleventh rib, counting from above; and at about four inches distance from the *vertebræ*. The direction of the incision was agreeable to the course of the ribs; and upon being made nearer to the superior edge of the eleventh rib, than to the inferior edge of the tenth rib, the intercostal artery by that means escaped being wounded. Upon dividing the intercostal muscles, very near twenty ounces of matter were discharged, after which I introduced

introduced my finger thro' the wound into the cavity of the *thorax*, but found no adhesion of the lungs. From whence I am inclined to conjecture, that this abscess was originally formed in the cellular membrane of the *pleura*, which had at length made its way into the cavity.

What seems to corroborate this conjecture, is, that the violent symptoms, which happen'd upon lying on the sound side, or upon sitting upright, did not occur till within a week before his application to me.

From the moment the matter was discharged, he found immediate ease, his respiration became quiet; his fever and cough gradually abated, till in about six weeks he became perfectly well in all respects, and was accordingly dismiss'd the hospital.

The discharge from the wound continued in considerable quantities for the first fortnight; during which time the wound was kept properly open with tents: but when the discharge was no more than what might be expected from any superficial wound of the same size, all tents were disused, and superficial applications only made use of.

LXVII. *An Account of the Eruption of Mount Vesuvius in Oct. 1751, in a Letter to Sir Matthew Fetherston-Haugh, Bart. F.R.S. written at Naples Jan. 15, 1752, N. S.*

Read Mar. 19, 1752. **T**HAT, which has taken up our attention, and astonish'd us most, is the eruption of Mount Vesuvius. As it was a surprising prodigy of nature, I shall, for y<sup>o</sup>ur amusement,

F f f

filk

fill up the remainder of this paper with what I remark'd, and was informed of relative to it.

I was several times to see it. The inhabitants round the foot of the mountain told us, that they felt several shocks of an earthquake a day or two before the eruption ; as also several loud reports in different places of the mountain, like the firing of cannon, but louder. The top of it smok'd much more than usual, and was mix'd with streams of flame. The bottom of the great *crater*, which was before an indurated scurf of bitumen and sulphur, is now full of large rents or openings, cover'd over with sal armoniac, nitre, and sulphur. The little mountain, from whence, before this eruption, the smoke and flame issued, and which was within the great *crater*, is now intirely sunk down, and a horrible fiery gulph appears where it stood. We could not approach it so near as to look down, being prevented by the smoke and fiery matter which it threw out incessantly. The concreted scurf at the bottom was liquefied and boiling in several places ; particularly from the fiery gulph to that part of the side of the mountain, whence the eruption broke out, a canal was sunk down, in breadth some feet.

On the 25 of October, in a place call'd Atrio del Cavallo, on the east side of the mountain, a fiery fluid, like melted glass in a furnace, burst out, or rather seemed to boil over, which ran down the declivity of the mountain with great velocity and force, carrying along with it large stones, gravel, calcin'd earth, &c. In six hours time it ran four miles, and cover'd vast tracts of fine land ; destroy'd many farm-houses, villa's, and vineyards. It is computed to have

have done damage to the value of at least 60,000 ducats. The reason, why it does so much mischief, is, that it spreads itself, where the ground is plain, and covers in some places above an acre in breadth : But where there is a hollow ground, it forms a current river, making banks of its own substance, by cooling and hardening towards the edges ; and when this current happen'd to be oppos'd by a rising ground, (the high banks of the cooling lava preventing its passage on either side) it formed high mountains of lava of 50 or 60 feet ; till at last, by the weight and force of the red-hot river flowing incessantly from the Pocca above, it burst out from under this new hill, and forming a second fiery river, proceeded down the country, destroying all where it came.

It would affect you to see the poor inhabitants crying, and lamenting their irreparable losses ; and it was shocking to see trees, and vines loaded with fruit, floating upon this river of fire. And, to our great astonishment, tho' we plainly saw the fluidity and rapid current of this matter, yet was it so impenetrable, that no weighty body would sink in it ; nor did a sharp heavy iron instrument, thrown at it with great force, make the least impression on it, but, remaining on it a few minutes, it became red-hot like the lava. Nor could the pious procession and liquefaction of St. Januarius's blood upon the spot put a stop to the destructive inundation ; for it has run these two months past, and runs a little as yet. The whole is such a stupendous prodigy of nature, as must puzzle the wisest philosophers to account for. Why does this subterraneous caldron boil over only at certain periods of time ? And whence is it

supplied with combustible *pabulum* for many hundreds or thousands of years?

LXVIII. *An Account of an Hydrophoby, by*  
Thomas Wilbraham, LL D. F. R. S.

Read April 9, 1752. ON Sunday March 29, 1752, Isaac Cranfield, a waterman, about 30 years of age, was received into the infirmary in Westminster, with an *hydrophobia* upon him. He had been that morning with Mr. Heathfield, one of the surgeons to that infirmary, for advice; who being inform'd of that remarkable symptom, ask'd him, if he had not been lately bitten by a dog? He answer'd, no. But his wife, who was with him, put him in mind, that he had received a wound from a dog about nine months before. This he presently recollected; and said, it was a strange dog he met with at a public-house, that, as he was going to stroak him, gave him a little bite in the hand.

The same day, about one o' clock, Dr. Coxe, Dr. Watson, and myself, who are join'd in the care of the above-mention'd infirmary, met together there to consult upon his case. When he came to be examin'd, he repeated to us the manner of his being bitten, as just mentioned; and said further, that he no sooner found himself hurt, but he gave the dog such a blow with a poker, as laid him dead upon the spot.

The wound, being slight, soon heal'd up, and he thought no more of it; and he enjoy'd good health  
till

till about two o'clock the Thursday morning before, when he was seized with a violent sickness and vomiting. The day following he continued very ill, and particularly felt an unusual pain, whenever he attempted to drink. Friday and Saturday that symptom grew worse; and on Sunday he could not swallow the least quantity of liquor, without the utmost misery.

This was the day we saw him. He look'd somewhat wild in his eyes; but, in his discourse with us, discover'd no signs of madness. His pulse was extremely quick, but not weak and depressed. We examin'd his *fauces*, and found an inflammation. We desir'd him to give us an opportunity to see how he could bear an attempt to get down some liquid. He readily consented. He chose to sit down upon the floor, then took a cup of water in his own hand, and put it to his mouth. The moment the liquor reach'd his throat, he suddenly sprung up on his feet, and ran about the room in the most violent agony, that can be conceiv'd. It must be observ'd, that he could get down small quantities of food that was solid, all the time this symptom was upon him.

He informed us, he had been let blood twice the day before he came to us. We agreed to take from him 12 ounces more, and to give him a grain of *extractum Thebaicum* every hour, till there appear'd some signs of *stupor* from the medicine. We likewise order'd him a clyster of *decoct. furfuris* with nitre. The blood was found next day not differing from that of a person in health. The extract was made up in pills of a grain each, which he could swallow without difficulty. I saw him again at eight o'clock

o'clock at night, at which time he had taken five grains of opium, but did not appear to be in the least affected by it, being much in the same state I had left him in at one. He had had the clyster twice, but no stool either time. He went on with the pills till he had taken 15 grains; but no effect could be perceived from them. He passed the night in great anxiety, being for the most part on his legs, and at times light-headed. A good deal of frothy saliva was discharged from his mouth.

About 8 o'clock in the morning he died. A few minutes before he expir'd, he said, that he was sensible he was going to die; and express'd much concern for the loss, which his wife and children would have of him.

That day we had him open'd. The lungs were found full of blood. Water in the *pericardium* in the usual quantity. The blood in both ventricles of the heart fluid. The *œsophagus* without any morbid appearance (*Vide Boerhaave Aphor. 1140.*) The *aspera arteria* full of such frothy substance as came from his mouth. The stomach fill'd with liquor, notwithstanding the small quantity he had drank since Wednesday evening. No other parts were examined.

April 7, 1752.

**LXIX.** *A Letter from Mr. J. Smeaton to Mr. John Ellicott, F.R.S. concerning some Improvements made by himself in the Air-Pump.*

S I R,

Read April 16, 1752. **I** HAVE been informed by some of my friends, that my endeavours towards completing the air-pump, have been mentioned with approbation, in papers that Mr. Short and Mr. Watfon have lately communicated to the Royal Society. I understand likewise, that the latter of those gentlemen has, in a very obliging manner, expressed an inclination, that I should lay before them a particular account of my improvements therein.

I shall always esteem it a singular honour to be thought capable of producing any thing worthy the attention of the Royal Society ; and to be my duty and interest so to do, upon the least intimation of that kind.

Your superior skill in mechanics, together with the assistance you have given me in making trial of my pump, against three very good ones of the common construction, as well as the frequent marks of friendship you have shewn me on all occasions, encourage me to trouble you with communicating the following to that Society, of which you are a member, and who, of all others, are the most proper judges.

I shall



I shall not take up time with a particular recital of the alterations I have made, for near four years past, in order to remove some obstacles, which I imagined hindered the effects, that the theory I set out upon seemed to promise. It will be sufficient, that I give an account of what has appeared to answer best, after a great number of different trials; which, tho' short of what I at first expected, yet as this pump performs much better than the common ones, my labour may not be thought wholly useless; and the respect, which I have to the Society, would still have prevented me from troubling you or them about it at this time, could I have thought of any alteration, that promised materially to improve it.

The principal causes of imperfection in the common pumps arise, first, from the difficulty in opening the valves at the bottom of the barrels; and, 2dly, from the piston's not fitting exactly, when put close down to the bottom; which leaves a lodgement for air, that is not got out of the barrel, and proves of bad effect, as I shall shew in the course of this paper.

In regard to the first of these causes; the valves of air-pumps are commonly made of a bit of thin bladder, stretch'd over a hole generally much less than one tenth of an inch diameter; and to prevent the air from repassing between the bladder and the plate, upon which it is spread, the valve must always be kept moist with oil or water.

It is well known, that at each stroke of the pump the air is more and more rarefied, in a certain progression, which would be such, that an equal proportion

proportion of the remainder would be taken away, was it not affected by the impediments I have mentioned: so that, when the spring of the air in the receiver becomes so weak, as not to be able to overcome the cohesion of the bladder to the plate, occasioned by the fluid between them, the weight of the bladder, and the resistance that it makes by being stretch'd, the rarefaction cannot be carried farther, tho' the pump should still continue to be worked.

It is evident, that the larger the \* hole is, over which the bladder is laid, a proportionably greater force is exerted upon it by the included air, in order to lift it up; but the aperture of the hole cannot be made very large, because the pressure of the incumbent air would either burst the valve, or so far force it down into the cavity, as to prevent its lying flat and close upon the plate, which is absolutely necessary.

To avoid these inconveniences as much as possible, instead of one hole, I have made use of seven, all of equal size and shape; one being in the centre, and the other six round it: so that the valve is supported at proper distances, by a kind of grating, made by the solid parts between these holes: And to render the points of contact, between the bladder and grating, as few as possible, the holes are made hexagonal, and the partitions filed almost to an edge. As the whole pressure of the atmosphere can never be exerted upon  
this

\* If we examine, the force, that air rarefied 140 times can exert in a common valve through a hole of one tenth of an inch diameter, we shall find it not to exceed 6 grains at a medium.

this valve, in the construction made use of in this pump; and as the bladder is fastened in four places instead of two, I have made the breadth of the hexagons three tenths of an inch; so that the surface of each of them is more than nine times greater than common. But as the circumference of each hole is more than three times greater than common, and as the force, that holds down the valve, arising from cohesion, is, in the first moment of the air's exerting its force, proportionable to the circumference of the hole; the valve over any of these holes will be raised with three times more ease than common. But as the raising of the valve over the center-hole is assisted on all sides by those placed round it; and as they all together contribute as much to raise the bladder over the center-hole, as the air immediately acting under it; upon this account the valve will be raised with double the ease, that we have before supposed, or with a sixth part of the force commonly necessary.

It is not material to consider the force of the cohesion, after the first instant: For, after the bladder begins to rise, it exposes a greater surface to the air underneath, which makes it move more easily. I have not brought into this account the force, that keeps down the valve, that arises from the weight of the bladder, and the resistance from its being stretch'd; for I look upon these as small, in comparison of the other.

I was not however contented with this construction of the valves, till I had tried what effect would be produced, when they were opened by the motion of the winch, independent of the spring of the air:

And tho' the contrivance I made use of seemed to me less liable to objection than any thing I was acquainted with, that had been designed for that purpose ; yet I did not find it to answer the end better than what I have already described ; and therefore laid it aside, as it render'd the machinery much more complex, and troublesome to execute.

But supposing all those difficulties to be absolutely overcome, the other defect, that I mentioned in the common construction would hinder the rarefaction from being carried on beyond a certain degree. For, as the piston cannot be made to fit so close to the bottom of the barrel, as totally to exclude all the air ; as the piston rises, this air will expand itself, but still pressing upon the valve, according to its density, hinders the air within the receiver from coming out : Hence, were this vacancy to equal the 150th part of the capacity of the whole barrel, no air could ever pass out of the receiver, when expanded 150 times, tho' the piston was constantly drawn to the top ; because the air in the receiver would be *in æquilibrio* with that in the barrel, when in its most expanded state. This I have endeavoured to overcome, by shutting up the top of the barrel with a plate, having in the middle a collar of leathers, through which the cylindrical rod works, that carries the piston. By this means, the external air is prevented from pressing upon the piston ; but that the air, that passes thro' the valve of the piston from below, may be discharged out of the barrel, there is also a valve applied to the plate at the top, that opens upwards. The consequence of this construction is, that when the piston is put down to the bottom of the cylinder, the air in the lodgment under the piston will evacuate

itself so much the more, as the valve of the piston opens more easily, when pressed by the rarefied air above it, than when pressed by the whole weight of the atmosphere. Hence, as the piston may be made to fit as nearly to the top of the cylinder, as it can to the bottom, the air may be rarefied as much above the piston, as it could before have been in the receiver. It follows therefore, that the air may now be rarefied in the receiver, in duplicate proportion of what it could be upon the common principle; every thing else being supposed perfect.

Another advantage of this construction is, that tho' the pump is composed of a single barrel\*, yet the pressure of the outward air being taken off by the upper plate, the piston is worked with more ease § than the common pumps with two barrels: And not only so, but when a considerable degree of rarefaction is desired, it will do it quicker; for the terms of the series expressing the quantity of air taken away at each stroke do not diminish so fast, as the series answering to the common one.

I have found the gages, that have been hitherto made use of, for measuring the expansion of the air, very unfit to determine in an experiment of so much nicety.

\* It is obvious that these improvements will equally obtain, whether the pump is constructed with a single or a double barrel.

§ Because, tho' the pressure of a column of air, equal to the diameter of the piston-rod, still presses upon it, yet, as there is only the friction of one piston, and that not loaded with the weight of the atmosphere; the friction of the leather against the side of the barrel, and that of the rack and wheel, is much less: so that, notwithstanding the addition of friction in the collar of leathers, that of the whole will be less.

nicety. I have therefore contrived one of a different sort, which measures the expansion with certainty, to much less than the 1000th part of the whole. It consists of a bulb of glass something in the shape of a pear, and sufficient to hold about half a pound of quicksilver. It is open at one end, and at the other is a tube hermetically closed at top. By the help of a nice pair of scales, I found what proportion of weight a column of mercury, of a certain length, contained in the tube, bore to that, which filled the whole vessel. By these means I was enabled to mark divisions upon the tube, answering to a 1000th part of the whole capacity, which being of about one tenth of an inch each, may, by estimation, be easily subdivided into smaller parts. This gage, during the exhausting of the receiver, is suspended therein by a slip-wire. When the pump is worked as much as shall be thought necessary, the gage is pushed down, till the open end is immersed in a cistern of quicksilver placed underneath: The air being then let in, the quicksilver will be driven into the gage \*; till the air remaining in it becomes of the same density with the external; and as the air always takes the highest place, the tube being uppermost, the expansion will be determined by the number of divisions occupied by the air at the top.

The degree, to which I have been able to rarefy the air in experiment, has generally been about 1000 times,

\* The bulb of the gage may be emptied of its quicksilver, without taking that out of the tube; and the tube being held horizontal, the column of mercury in it will have power to contract or expand the air at the top.

times, when the pump is put clean together : But the moisture, that adheres to the inside of the barrel, as well as other internal parts, upon letting in the air, is in the same succeeding trials worked together with the oil, which soon renders it so clammy, as to obstruct the action of the pump upon a fluid so subtil as the air is, when so much expanded ; but in this case it seldom fails to act upon the air in the receiver, till it is expanded 500 times : And this I have found it to do, after being frequently used for several months, without cleaning. I have also generally found it to perform best, the first trial at each time of using ; tho' nothing had been at it from the time preceding ; which, after a great many trials made with this view, I also attribute to the vapours of the air mixing with the oil. An experiment, where the air was expanded 1000 times, was tried about two years since in your presence ; at which were present also Dr. Knight and Mr. Canton ; and I lately did the same thing with Mr. Watson. The pump, which I intend myself the honour of shewing the Society, is the same, that I just now mention'd, and the second that I made, with a view to improve upon this principle.

The degree of rarefaction, produced by the best of the three pumps, that you procured the trial of, and which you esteemed good in their kind, and in complete order, never exceeded 140 times, when tried by the gage above described.

I have also endeavoured to render the pneumatic apparatus more simple and commodious, by making this air-pump act as a condensing engine at pleasure, by singly turning a cock. This not only enables us to

to try any experiments under different circumstances of pressure, without changing the apparatus, but renders the pump an universal engine, for shewing any effect, that arises from an alteration in the density or spring of the air. Thus, with a little addition of apparatus, it shews the experiments of the air-fountain, wind-gun, &c.

This is done in the following manner: The air above the piston being forcibly driven out of the barrel at each stroke, and having no-where to escape, but by the valve at the top; if this valve be connected with the receiver, by means of a pipe, and at the same time the valve at the bottom, instead of communicating with the receiver, be made to communicate with the external air, the pump will then perform as a condenser.

The mechanism is thus ordered. There is a cock with three pipes placed round it, at equal distances. The key is so pierced, that any two may be made to communicate, while the other is left open to the external air. One of these pipes goes to the valve at the bottom of the barrel; another goes to the valve at the top, and a third goes to the receiver. Thus, when the pipe from the receiver, and that from the bottom of the barrel, are united, the pump exhausts: But turn the cock round, till the pipe from the receiver, and that from the top of the barrel, communicate, and it then condenses. The third pipe, in one case, discharges the air, taken from the receiver, into the barrel; and in the other, lets it into the  
barrel.



barrel, that it may be forced into the receiver. I  
am,

S I R,

Furnival's-Inn-court,  
April 16, 1752.

Your most humble servant,

J. Smeaton.

*P. S.* I have also added some draughts, and letters of reference, in order to explain myself more fully.

### Figure I.

Is a perspective view of the principal parts of the pump together.

*A* is the barrel.

*B* the cistern, in which are included the cock, with several joints. These are cover'd with water to keep them air-tight. A little cock to let the water out of the cistern, is marked 6.

*Ccc* is the triangular handle of the key of the cock: which, by the marks on its arms, shews how it it must be turned, that the pump may produce the effect desired.

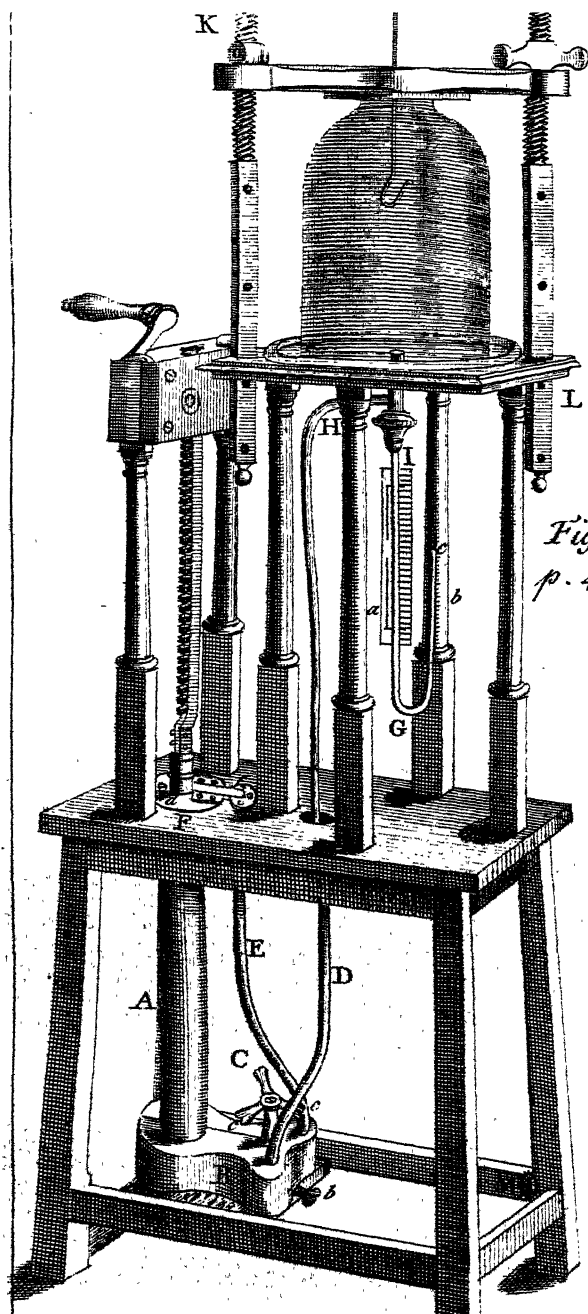
*DH* is the pipe of communication between the cock and the receiver.

*E* is the pipe, that communicates between the cock and the valve, on the upper plate of the barrel.

*F* is the upper plate of the pump, which contains the collar of leathers *d*, and *V* the valve, which is covered by the piece *f*.

*GI*





*Fig. 1.*  
*p. 424.*

. 4. p. 427.

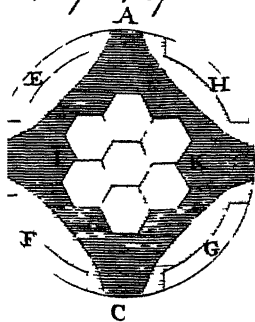


Fig. 5.  
p. 428.

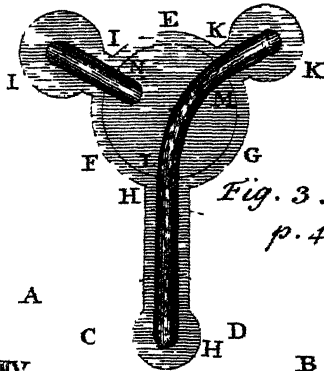
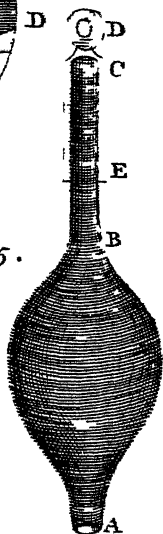
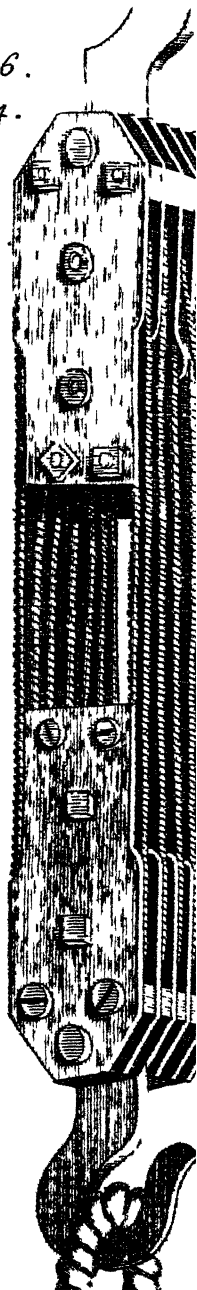


Fig. 3.  
p. 426.



2 rows. 17 rows. 1 row.

Fig. 6.  
p. 494.





*GI* is the siphon-gage; which screws on and off, and is adapted to common purposes. It consists of a glass tube hermetically sealed at *c*, and furnished with quicksilver in each leg; which, before the pump begins to work, lies level in the line *ab*; the space *bc* being filled with air of the common density. When the pump exhausts, the air in *bc* expands, and the quicksilver in the opposite leg rises, till it becomes a counter-balance to it. Its rise is shewn upon the scale *le*, by which the expansion of the air in the receiver may be nearly judged of. When the pump condenses, the quicksilver rises in the other leg, and the degree may be nearly judged of by the contraction of the air in *bc*: marks being placed at  $\frac{1}{2}$  and  $\frac{2}{3}$  of the length of *bc* from *c*; which shews when the receiver contains double or treble its common quantity.

*KL* is a screw-frame to hold down the receiver, in condensing experiments, which takes off at pleasure; and is sufficient to hold down a receiver, the diameter of whose base is 7 inches, when charged with a treble atmosphere: in which case it acts with a force of about 1200 pounds against the screw-frame.

*M* is a screw, that fastens a bolt, which slides up and down in that leg, by means whereof the machine is made to stand fast on uneven ground.

### Fig. II.

Is a perpendicular section of the barrel and cock, &c. where

*AB* represents the barrel.

H h h

*CD*

*CD* the rod of the piston, which passes through  
*MN* the plate, which closes the top of the barrel.  
*K* is the collar of leathers, through which the piston-rod passes. When the piston is at the bottom of the cylinder, the upper part of *K* is covered by the cap at *D*, to keep out dust, &c.  
*L* is the valve on the upper plate, which is covered by the piece  
*OP*, which is connected with the pipe  
*QR*, which makes the communication between the valve and cock.  
*CE* is the piston; and  
*EFF* is the piston-valve.  
*II* are two little holes to let the air pass from the piston-valve into the upper part of the barrel.  
*GGK* is the principal valve at the bottom of the cylinder.  
*HH* is a piece of metal, into which the valve *GGK* is screw'd, and closes the bottom of the cylinder; out of which also is composed  
*SS* the cock, and  
*KTT* the duct from the cock to the bottom of the barrel.  
*WW* is the key of the cock.  
*X* the stem; and  
*VV* the handle.

Fig. III.

Is an horizontal section of the cock, through the middle of the duct *TT*.

*AB* represents the bigness of the circular plate, that closes the bottom of the barrel.

*CD*

*CD* represents the bigness of the inside of the barrel. *EFG* is the body of the cock; the outward shell being pierced with 3 holes at equal distances, and corresponding to the three ducts *HH*, *II*, *KK*, whereof

*HH* is the duct, that goes to the bottom of the barrel. *II*, the duct, that communicates with the top of the barrel; and

*KK* is the duct, that passes from the cock to the receiver.

*LMN* is the key, or solid part of the cock, moveable round in the shell *EFG*. When the canal *LM* answers to the ducts *HH* and *KK*, the pump exhausts, and the air is discharged by the perforation *N*. But the key *LMN* being turned till the canal *LM* answers to *II* and *KK*, the perforation *N* will then answer to *HH*; and in this case the pump condenses. Lastly, when *N* answers to *KK*, the air is then let in or discharged from the receiver, as the circumstance requires.

#### Fig. IV.

Is the plan of the principal valve.

*ABCD* represents the bladder fasten'd in 4 places, and stretch'd over the 7 holes *IK*, formed into an hexagonal grating; which I shall call the honey-comb.

*EFGH* shews where the metal is a little protuberant, to hinder the piston from striking against the bladder.



Fig. V.

Represents the new gage; which I call the pear-gage. It is open at *A*; *BC* is the graduated tube, which is hermetically closed at *C*, and is suspended by the piece of brass *DE*, that is hollowed into a cylinder, and clasps the tube.

LXX. *An Account of Aphyllon and Dentaria heptaphyllos of Clusius, omitted by Mr. Ray: by Mr. William Watson, F. R. S.*

Read April 16,  
1752.

**M**R. Watson presented to the Society some specimens of two plants, now in flower, which he said were not frequently found in England. One of them was the *Anblatum* of Cordus, or *Aphyllon* of John Bauhin. This plant is denominated *Squamaria* by Rivinus, and *Dentaria crocodylia* by Tabernamontanus. Linnæus, in the *Flora Suecica*, calls it *Lathræa caule simplicissimo, corollis nutantibus, labio inferiore trifido*. Mr. Ray, in his *Synopsis plantarum Angliæ*, takes notice of its being found near Dorking in Surrey, but the plant now presented was collected near Harefield in Middlesex.

The other plant offered was the *Dentaria heptaphyllos baccifera* of Caspar Bauhin, or *Dentaria tertia baccifera* of Clusius. This plant is treated of by Linnæus, in the *Hortus Cliffortianus*, and by Van Royen, in the *Floræ Leydenfis prodromus*, under the appellation of *Dentaria foliis inferioribus palmatis, summis simplicibus*.

This

This plant, which is frequently met with upon the continent of the northerly parts of Europe, has been but lately discover'd to grow in England, and that only in one place; *viz.* in a wood not far from Harefield in Middlesex, where it was first discover'd by Mr. Blackstone, an ingenious apothecary in Fleet-street. This is one of those few plants omitted by the late Mr. Ray in his excellent *Synopsis*, which are found to be natives here; and, from their great scarcity, it is not wonderful, that they were unobserved by that great naturalist.

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LXXI. *An Account of a Machine for killing of Whales, proposed by John Bond, M D.*

Read April 23, 1752. **B**EFORE I give a particular description of this machine, I humbly beg leave to premise some account of the present method of killing whales, which I have collected from several persons of credit, who have been employ'd at Greenland, that those, who are unacquainted with it, may see, how dangerous and uncertain it is, and how much some improvement is requir'd to render it more successful.

Whales being of the same structure internally with quadrupeds, must come frequently to the surface of the water to breathe; and when they expel the rarefied air from their capacious lungs, thro' a narrow tube, which protrudes above the upper jaw, they occasion a great noise, which the  
fishers

fishers term *the blowing of the whale's*. This noise alarms the fishers, who are waiting for that signal; upon which they furnish a boat with necessary instruments, and row quietly towards the whale. The harpooneer, as they call him, sits rowing in the head of the boat, and observes certain silent signals, which the boat-steerer gives him, to inform him, that he is near enough to strike the whale. Then the harpooner takes the harpoon in both hands, and darts it into the whale; which, as soon as struck, plunges directly to the bottom, and moves with such prodigious velocity, that the rope, which follows the harpoon, often cuts deep grooves in the boat, and a man stands ready with an ax to cut the rope, if it does not run freely from the coil. The whale being hurt by the harpoon, stays longer than usual under water, till the blood, by the violent motion of the body, is collected about the heart, and consequently obstructed in the head; the nervous influx is interrupted, the swimming bladder relaxed, and the whale becomes languid, and rises to the top to breathe fresh air, and rests for some time, to recruit its exhausted spirits; which the fishers observing, row up and dispatch the whale with long lances.

It appears from this account, that the greatest difficulty consists in making the rope fast to the whale, by means of the harpoon; which is barbed in the common form of a dart, and is generally 20 ounces weight, and about two feet long, with a small stalk of flexible iron, and a socket at the end, about which the rope is spliced with a shaft of wood put into it, so that they cannot throw it any distance with any degree of certainty; therefore are never sure of  
 darting

darting a whale, till they are within a yard, or directly above her; and there they are so much afraid of being dash'd to pieces, that they often miss good opportunities, tho' they seldom meet with any so tame. They frequently see forty whales within thirty yards of their boats, but cannot strike one, unless it be sleeping, or suckling its young ones. Hence the bad success, and necessity of giving a premium to indemnify the adventurers. Hence we also see, that a machine, which would project a harpoon thirty yards with sufficient force and proper direction, must give a chance for giving thirty whales for one in the common way.

Several machines have been proposed to answer this end, but have all prov'd abortive. The cross-bow was try'd, but was too weak, and subject to break with the frost in those cold climates.

Gunpowder was next applied, I am told with no better success; for, besides the difficulty of applying it to throw those heavy bodies in the form of darts, especially such, as must carry a rope along with them, it frightens all the whales from the place, where it is fired, either by the light, or by the explosion, which it produces; perhaps, both ways; but I imagine, more by the sound than the light; for, in the summer-time, there is in those parts a continual day for several months, so that a flash would not be remarkable. I know it is doubted by the best physiologists, that fishes can hear, or that water, being incompressible, can propagate sonorous undulations below the surface. Notwithstanding the several ingenious experiments, that prove water incompressible, yet there are several facts, that seem to contradict  
that

that conclusion; such as the reflexion of hard bodies, which impinge obliquely on the surface of water; which shews, that water is elastic, and therefore compressible. It will be objected, that the parts of the hard bodies are compressed, not those of the water: but I suspect, if the same means be used to compress a crystal ball, a piece of diamond, or hard steel, we shall have the same reason to conclude them incompressible also; tho' I am sure, that they will all rebound from water, if they impinge at any angle under fifteen degrees.

I do not doubt the truth of these ingenious experiments, *viz.* that they could not reduce water by any force, which they used, to less bulk; but we have sufficient reason to doubt, that water is an absolutely hard body. To determine by a fair experiment, if sound could be convey'd under water, I desired an acquaintance to stand on the bank of a river, till I divid about three feet under water; then to pronounce any words he pleased in a pretty strong voice. These words I heard distinctly under water, and repeated them, when I raised my head above the water; which proves, that sound is convey'd under water, and that fishes may hear, if they have proper organs. In most fishes, which I have examin'd, there are perforations between the eyes and the extremity of the upper jaw, not in the middle, but rather nearer the eyes. Below those holes in the skin, is a pretty large cavity, at the bottom of which is a fleshy substance, which is richly supplied with nerves, by a thick medullary cord, which rises from the anterior lobes of the brain, and passes through the hinder-part of the orbit of the eye, where it divides into several branches,  
some

some of which are distributed to the parts about the extremity of the upper jaw, and one large branch is lost in the substance at the bottom of the cavity above describ'd, which I take to be the organ of hearing. This large nerve has been call'd by several authors the olfactory nerve of fishes; but I think it might with more truth and propriety be consider'd as a collection of the nerves, which supply the organs of tasting, smelling, and hearing, confin'd in a sheath, which is a production of the integuments of the brain. From the whole I would conclude, that fishes do hear; and that therefore gunpowder is unfit for projecting harpoons. The machine, which I would recommend for that purpose, is the antient *Balista*, which is accurately describ'd in the thirteenth chapter of Polybius, translated into French by Mons. Folard, who has nicely distinguish'd it from the *Catapulta*, with which most of the antient historians have confounded it, though these machines had distinct officies; for the *Catapulta* threw vast masses of metal and stone in a parabolic curve, and the *Balista* projected darts, some of sixty pounds weight, in a horizontal direction. The projectile power of both these machines depended upon twisted ropes, which mov'd a lever plac'd in their center. In the *Catapulta* this lever mov'd vertically, and threw off globular bodies, as above-mention'd; but in the *Balista* there were two levers, which mov'd horizontally, and acted like a cross-bow. It is needless to enter farther into a description of it, as I have sent an exact model of it to the repository of this Society, to which I refer any gentleman, that has a curiosity to see it.

I was obliged to differ from the antient plan, in adapting it to the harpoon, and have substituted hair-ropes instead of hemp, which the antients used. I have also contrived a new lock, much simpler than any of the cross-bow kind, recommended by Folard. By various experiments I found hairs to be very durable elastic substances, that cannot be sensibly affected by any degree of heat or cold, which the human body can bear. I stretch'd a single hair on a fiddle three inches beyond its natural length; let it stand 24 hours; then relax'd it, and it soon retracted to its former length. A single hair is neither elongated nor contracted by lying in water; but a number of hairs twisted together are shorten'd; which is owing to the attraction of the surfaces, not to the absorption of the supposed internal cavities of the hairs, the existence of which is dubious.

The force of this machine may be increased to any necessary degree, by multiplying the number of springs or ropes, and increasing the length of the lever, which turns the windlace, that draws back the cross cord, or in other words charges it. It has all necessary motions, and is contrived to stand on a pedestal in the head of a boat. It is so simple, that any person may learn how to use it in a short time; and when once it is successfully applied, we shall be no longer obliged to the instruction of the Dutch, who reckon it their interest to obstruct our success in every useful branch of trade.

When the ingenious and benevolent members of this learned Society, have consider'd the importance of this machine, and how far it may promote the public interest, I hope they will recommend the use of it  
to

to those, who are concern'd in the whale-fishery; it being foreign to my profession to prosecute the application of it, farther than to give a hint; and also to my inclination, to persecute the government for patents or premiums, according to the modern mercenary custom, leaving such acknowledgments to the public generosity.

I know the application of this machine will be strenuously opposed by the harpooners, because one machine might do more execution than an hundred of them; besides that the ignorant part of mankind has a strong prejudice against all improvements, and a rigid attachment to old methods.

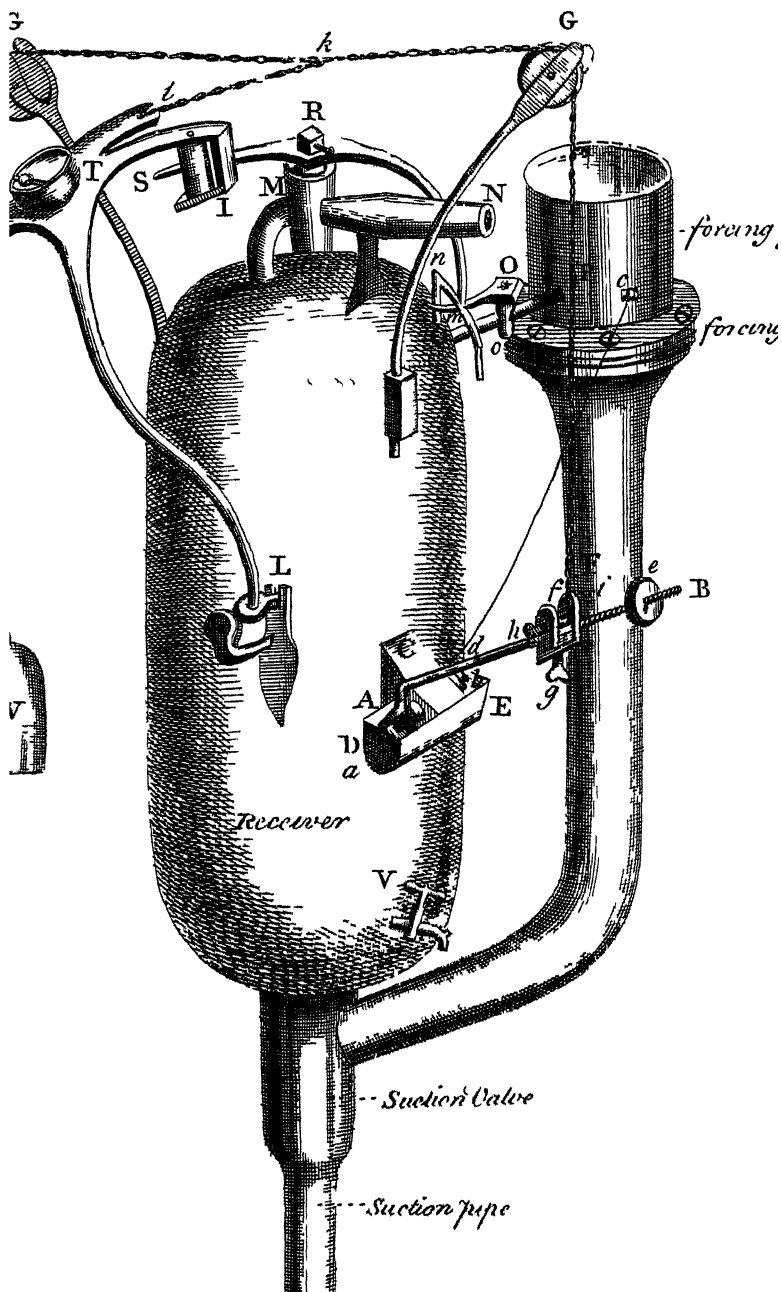
A Dutch captain, who had been many years at Greenland, told me, that, if he had a machine, which would throw a harpoon fifteen yards with sufficient force, he would soon load his ships: but said, that it was impossible to find out such a machine, because his countrymen did not know it.



**LXXII.** *An Engine for raising Water by Fire; being an Improvement of Savery's Construction, to render it capable of working itself, invented by Mr. De Moura of Portugal, F. R. S. described by Mr. J. Smeaton.*

Read Nov. 9, 1752. **T**HIS engine consists of a receiver, a steam and an injection-cock; a suction and a forcing-pipe, each furnished with a valve; together with a boiler, which, on account of its bulk and weight, is not sent with the rest; but, as it may be of the common globular shape, and having nothing particular in its construction, a description of it will not be necessary, as also the rest of these parts already mentioned being essential to every machine of this kind, a further account of them may be dispensed with. What is peculiar to this engine is a float within the receiver, composed of a light ball of copper, which is not loose therein, but fastened to the end of an arm, which is made to rise and fall by the float, while the other end of the arm is fasten'd to an axis; and, consequently, as the float moves up and down, the axis is turned round one way, or the other. This axis is made conical, and passes through a conical socket; which last is folder'd to the side of the receiver. Upon one of the ends of the axis, which projects beyond the socket, is fitted a second arm, which is also moved backward and forward by the axis, as the float rises or falls. By these means, the rising or falling of the surface of the water within the receiver communicates a correspondent motion to the outside, in order to  
give









give proper motions to the rest of the gear, which regulates the opening and shutting of the steam and injection-cocks; and serves the same purpose as the plug-frame, &c. in Newcomen's engine. The particular construction, and relation of those pieces, will better appear by the figure and references, than can be done by a general description.

*AB* an arm, which is fastened to

*ab*, a conical axis, which goes through a conical socket in

*C*, a triangular piece folder'd to the receiver. This piece has this shape, to give liberty to the arm to rise and fall, that carries the float on the inside.

*DE* is a small cistern, folder'd to the receiver; which, being kept full of water, keeps the axis and socket air-tight. This cistern is constantly kept full of water, by means of a small leakage through the wooden peg *c*, which follows the packthread *cd* to the cistern.

*e*, is a small weight to counterpoise the float within.

*f*, is a slider; which being set nearer to, or farther from, the axis, will rise, or fall, a greater or lesser space, as may be required; and is fastened by the screw *g*. This slider is furnish'd with a turn-about, *bi*, which is also fastened by a screw and nut at the end *i*, and serves to adjust the length of

*FGGH*, a chain, which gives motion, by means of the shorter chain *kl*, to

*IKL*, the balance, which opens and shuts the cocks; and moves upon the small axis *L*.

*GG* are two pulleys, supported by two arms, that are fasten'd to the side of the receiver, and give the chain,

chain a proper direction in order to move the balance.

*MN* is the steam-cock; the end *N* being supposed to be detached from a pipe, that gives it communication with the boiler.

*O* is the injection-cock, whose key is turned by the arm *Om*.

*PQ* is the injection-pipe, communicating between the forcing-pipe above the valve, and the top of the receiver.

*RS* is the arm, by which the key of the steam-cock is worked.

*IK* two rollers annexed to the balance, which, by striking upon the arm *RS*, open and shut the steam-cock, as the balance is moved backward and forward.

*Rno* is the steam-cock's key-tail, which is furnished with two small rollers, *n, o*, which open and shut the injection-cock, by acting upon the arm *Om* in such a manner, that, when the steam-cock is opened, the injection is shut, and *vice versa*.

*T* is a bell of advice, which, moving along with the balance, continues to ring as long as the engine is at work.

*V* is a cock, which serves to discharge the air from the receiver, and is open'd by hand, when necessary.

*W* is a weight sufficient to raise the balance to a perpendicular posture, when it is inclined to the right, and also to overcome the friction of the float, axis, pullies, chain, &c.

To put the engine in motion, press down the arm *AB*, which will bring the balance over to the right side, and in its motion will open the steam-cock, and shut

shut the injection ; set open the cock at *V*, that the air may be discharged by the entrance of the steam into the receiver. This being done, shut that cock, and let go the arm ; the weight *W* will bring over the balance to the left, and in its motion shut the steam-cock, and open the injection ; this presently condensing the steam into water, in a great measure leaves a *vacuum* in the receiver. Things remain in this situation, till the pressure of the atmosphere has caused the water to mount thro' the suction-pipe into the receiver, where, as its surface rises, it causes the float to ascend ; and, depressing the arm *AB*, raises the balance, till it has passed the perpendicular ; and, in its descent, which is done by its own gravity, the roller *K* lays hold of the arm *RS*, again opens the steam-cock, and shuts the injection. The receiver being now almost filled with water, the balance cannot return, till the surface of the water therein subsides, and suffers the float to descend. This is performed by the elasticity of the steam ; which, at the same time that it fills the receiver, drives out the water thro' the forcing-pipe ; and when the surface is descended so low, as to suffer the weight *W* to bring the balance beyond the perpendicular towards the left ; it then falls of its own accord, and, in falling, the roller *I* lays hold of the arm *RS*, shuts the steam-cock, and opens the injection, as before.

When the engine is desired to be stopp'd, observe, when the balance lies to the right, to turn round the arm *Om* of the injection-cock, so that the tail of the steam-cock may miss it in the next motion ; so that, at the same time that the receiver is fill'd with steam, and the steam-cock shut, the injection not being opened, the motion will stop for want thereof.



LXXIII. *A Letter from Dr. Parsons, F. R. S.  
to Mr. Peter Collinson, F. R. S. concern-  
ing the Shells of Crabs.*

Dear Sir, Red-lion Square, April 22, 1752.

Read April 30,  
1752

WHEN I had examined the crabs, sent you by Mr. Cook, I confess'd I had some doubts concerning them, which at present are clear'd up, by the last view I took of them. However, as I made no manner of question of the animal's casting his shell at certain seasons, your friend needed not be at the pains to quote so many authors, to prove what every naturalist knew before. I only wanted to be satisfied, that the old *exuviae* were those of the soft crab ; which the mutilated claw has indeed given me assurance of, however difficult it may be to conceive the manner of his quitting it.

It is no doubt a curious specimen, and, I hope, will be very convincing to your correspondent abroad, in support of a fact, which nobody, who has any pretence to natural knowledge among us, would hesitate about ; any more than we do of that animal's shaking off one or more limbs occasionally for his preservation. Nor is the manner of his acquiring a new limb in any wise different from that of his obtaining a succeeding new shell ; which is from a latent organization of the part ready for being indurated in due time, after the discharge of the old one ; at which time, and not before, the testaceous matter has room for its secretion thro' its proper emunctories.

This

This specimen is in every circumstance analogous to all the other animals, which annually cast their integuments; and, in its present soft state, resembles that of a hen's egg before the testaceous matter is secreted by the glands of the membrane; being soft and flexible: for this matter of all crustaceous animals, as well as of the eggs of fowls, is always successive to the intire formation of the membrane under it; nor are the glands capable of admitting the *minima* of the testaceous matter, till they have grown into a state proper for that purpose.

Hence it may be concluded, that the crab, lobster, or other such animal, which has this property, are, at first, furnished with this membrane intire, and sufficient to be a defence for the creature, against the violence of the agitated waves, and the rolling of sand, gravel, or other bodies, that might prove obnoxious to it, even before it can grow hard. This seems to be the method ordain'd by the Creator for the the preservation of every animal, however differing in other little circumstances. The snake, adder, lizard, or any other kinds, which we see endow'd with this property, have the new skin intire under the shrivell'd, falling, old one; and it is, no doubt, the case with crabs, lobsters, and other crustaceous animals.

In order to throw a little more light upon this matter, it may not be disagreeable to observe the manner of the induration of the surfaces of the shells of eggs.

It has been supposed, that these consist of a *mucus* indurated upon the surface of the membrane: but this is not the case. The particles of the shelly matter

matter are solid, tho' never so minute, and are carried with the fluids of the animal to the membrane, now ready to receive them into the ducts of its glands; and are thence thrown into such order in the cellules of the external surface, as to acquire a structure no less firm, in proportion, than bricks laid on one another; and as capable of bearing any fair pressure, as a well-built arch.

When they are thus hardened and complete, they may be render'd as soft and flexible, by being macerated in vinegar, as if the shelly particles had never been placed upon them. And this is not, because the matter is quite dissolved; for a vegetable acid is not capable of making a total dissolution of it; but the minute angles are destroy'd, and the particles (which were before fix'd like wedges to each other, to which they were inevitably guided in the secretion by the very structure of the receiving cellules of the membrane) are become round, by the destruction of their angles, and admit of being roll'd in some measure upon one another, so as in the whole to yield to the natural flexibility of the membrane.

I am, with great respect and friendship,

Your most humble servant,

James Parsons.

# LXXIV. Spherical Trigonometry *reduced to* Plane, *by* Francis Blake, *Esq. F. R. S.*

Read May 7, 1752. **I**T is observable, that the analogies of spherical trigonometry, exclusive of the terms co-fine and co-tangent, are applicable to plane, by only changing the expression, fine or tangent of side, into the single word, side\*: so that the business of plane trigonometry, like a corollary to the other, is thence to be inferr'd. And the reason of this is obvious; for analogies raised not only from the consideration of a triangular figure, but the curvature also, are of consequence more general; and tho' the latter should be held evanescent by a diminution of the surface, yet what depends upon the triangle, will nevertheless remain. These things may have been observed, I say; but upon revising the subject, it further occur'd to me, and I take it to be new, that from the axioms of only plane trigonometry, and almost independent of solids, and the doctrine of the sphere, the spherical cases are likewise to be solved.

Suppose, first, that the three sides of a spherical triangle,  $abd$  (Fig. 1.) are given to find an angle,  $a$ ; which case will lay open the method, and lead on to the other cases, in a way, that to me appears the most natural. It is allow'd, that the tangents,  $ae$ ,  $af$ , of the sides,  $ad$ ,  $ab$ , including an angle,  $a$ , make a plane angle equal to it; and it is evident, that the other side,  $db$ , determines the angle made by the secants  $ce$ ,  $cf$ , at  $c$  the centre of the sphere; whence the distance,  $ef$ , betwixt the tops of those secants, is

K k k                      given

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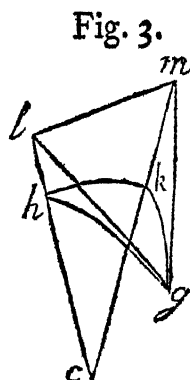
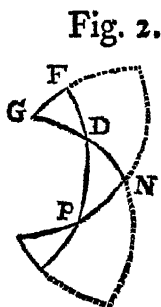
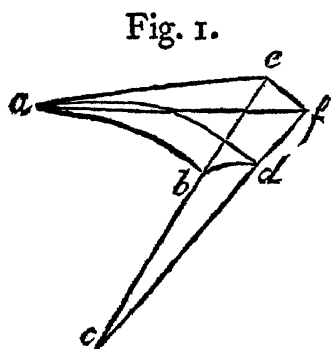
\* See M. De la Caille's remark at the end of the spherical trigonometry prefix'd to his Elements of Astronomy.

given by case the fifth of oblique plane triangles (see *Heynes's Trigonometry*.) which, with the aforesaid tangents, reduces it to case the 6th of oblique plane triangles also\*: and thus this 11th case of oblique triangles, so intricate hitherto, becomes perfectly easy. The 12th case is reducible to the 11th, and the rest, whether right-angled, or oblique, we are authorised to look upon as reducible to right-angled triangles, whose sides are not quadrants, but either greater or less than such. Conceive therefore, now, in a right-angled spherical triangle,  $gkb$  (Fig. 2.) that the tangent,  $gm$ , and secant,  $em$ , of either leg,  $gk$ , is already drawn; and in the point,  $m$ , of their union, draw a perpendicular,  $ml$ , to  $em$ , the secant, directly above the other leg, *viz.* a perpendicular to the plane of the secant and tangent, that it may be perpendicular to both (*Eucl.* 4, 11); for then will the tangent,  $gl$ , of the hypotenuse,  $gb$ , drawn from the same point, which that of the leg was, constantly terminate in the perpendicular line, that the radius and tangent may make a right-angle (*Eucl.* 18, 3). Whence these tangents,  $gm$ ,  $gl$ , and the perpendicular line,  $ml$ , together with the secants,  $cm$ ,  $cl$ , will evidently form two right-angled plane triangles,  $gml$ ,  $cm l$ ; and to one or other of these the spherical cases are easily transferr'd. Thus, if in the spherical triangle,  $gkb$ , the hypotenuse,  $gb$ , base,  $gk$ , and angle,  $g$ , at the base, be the parts given and required, when any two are given, the  
third

\* The angle to be found in this case must always be that formed by the two tangents.

third may be determined by means of a plane triangle; and at a single operation. We have, for instance, in the right-angled plane triangle,  $gml$ , formed as above, the base,  $gm$ , and hypotenuse,  $gl$ , to find, by case the 5th of right-angled plane triangles, the angle included, which is the same as on the sphere. And then if the base,  $gk$ , the angle,  $g$ , at the base, and perpendicular,  $hb$ , be the spherical parts given and required; or if the angles,  $g$  and  $h$ , and the hypotenuse,  $gb$ , be the parts given and required, we have only that former proportion of the hypotenuse and base, and angle at the base, in the triangles,  $PND$ ,  $DFG$ , obtained by the complements, to transfer to the plane. But secondly, suppose the spherical proportion is of the three sides, any two being given, the third may be also found at a single operation, in the second right-angled plane triangle,  $cm l$ , form'd as above. We have, for instance, the hypotenuse and base,  $cl$ ,  $cm$ , viz. the secant of the spherical hypotenuse and base  $gb$ ,  $gk$ , to find, by the 5th of right-angled plane triangles, the angle,  $c$ , at the center, which is the measure of  $kb$ , the side that was sought. And then again, if the hypotenuse, one leg, and the opposite angle be the spherical parts given and required; or if the two angles and a leg be the parts given and required, we have only the former proportion of the three sides in the triangles,  $PND$ ,  $DFG$ , obtained by the complements, to transfer to the plane. Whence, the six proportions of right-angled spherical triangles being comprehended in this method, it is fully demonstrated, that all the cases of these triangles are so to be resolved.

The same might be deduced without the method of complements, but neither in so short nor satisfactory a way, and it shall therefore be omitted. I have communicated this upon account of its perspicuity, and supposing, that in an age so greatly advanced in mathematical learning, the least hint of what is new would not be unacceptable.



Queen's Square, Westminster,  
May 7, 1752.

LXXV. *An Account of a manuscript treatise, presented to the Royal Society, intituled, Traité du corail, contenant les nouvelles decouvertes, qu'on a fait sur le corail, les pores, madrepores, scharras, litophitons, eponges, et autres corpset productions, que la mer fournit, pour servir a l'histoire naturelle de la mer; that is to say, A Treatise upon Coral, and several other Productions furnish'd by the Sea, in order to illustrate the natural History thereof, by the Sieur de Peyssonnel, M. D. Correspondent of the Royal Acad. of Sciences of Paris, of that of Montpelier, and of that of Belles Lettres at Marseilles; Physician-Botanist, appointed by His Most Christian Majesty in the Island of Guadalupe, and heretofore sent by the King to the Coasts of Barbary for Discoveries in Natural History. Extracted and translated from the French by Mr. William Watson, F. R. S.*

Read May 7, 1752. **T**HIS curious treatise before us, containing upwards of 400 quarto pages in manuscript, was transmitted to the Royal Society from Guadalupe. It is the result of the observations of above thirty years; and was sent hither, as M. de Peyssonnel informs the Society by a letter dated



dated at Guadalupe May 1, 1751, to be inserted, if it should be found worthy, in the *Philosophical Transactions*.

He does this, as he perceives, that in France some lovers of natural history do attribute and even appropriate to themselves his labours and his discoveries, of which they have had the communication; and that himself, retired to the West Indies, and not having the means of giving to his work the perfection he desired, for want of books, and yet more for want of judicious persons, with whom he might not only consult, but who might also enable him to give a more full explanation to such passages of his work, as might be thought obscure, and even correct the faults thereof; for which reason he takes the liberty to request this good office of the Royal Society.

This treatise is divided into two parts; the first of which relates to coral only, and is subdivided into ten chapters; to which is subjoined a catalogue of the remedies and compositions, as well chemical as galenical, in which coral is an ingredient. The second part is subdivided into eight dissertations, each of which has for its object some production of the sea; and the whole tends to evince, that as well coral, as the other marine bodies herein specified, and hereafter to be consider'd, are produced by animals, *viz.* different kinds of *urtica marina* & *purpura*. To these the author has added a complete index, referring to every thing taken notice of in the whole work.

This work is the result of a great number of very curious observations and inquiries, and has for its object a part of natural history not hitherto well known.

For

For the difficulties, which there are, in getting from the bottom of the sea its productions, and the few opportunities, which occur, of making the necessary observations upon sea-plants, have been the cause, that this part of botany has been hitherto very imperfect; and that the antients have been ignorant of the organisation and structure of these plants, of which they were acquainted but with a very small quantity, although the different species are exceedingly numerous.

M. Peyssonnel, disposed from his youth to the study of natural history, after having qualified himself for the practice of medicine, applied himself with great diligence to that science, to which his inclinations so strongly prompted him; and being a native of, and residing at Marseilles, he had the opportunity of examining the curiosities of the sea, which the fishermen, more especially those who search for coral, furnished him with. These considerations engaged him to endeavour to illustrate this obscure part of natural history, which he was more particularly enabled to do, as he could examine the productions of the sea the moment they were taken out of the water, or even in the sea itself, when these bodies are in their natural state: for most of those naturalists, who have treated of them already, have not examined them, but when they have been disfigured by the air, and have changed their true state by being dried. Besides, that tranquillity of mind, which a just observer should be always in possession of, is frequently disturbed in those little slight boats used by the coral-fishers. These, as well as several other difficulties, have been the cause, why we have so little knowledge

of the natural history of the sea. Our author found the means of overcoming these obstacles: the voyages which he made to the American islands, to St. Domingo, to Mississippi, to Egypt, and elsewhere, have accustomed him to the fatigues of the sea; and the frequent opportunities of embarking himself with the coral-fishers and others were very favourable to his purpose, and contributed to the discoveries, which he made upon this subject, and which he verified and enlarged, when in Barbary by the king's orders. As coral, next to pearls and ambergrise, was the most precious marine production, it was not to be wonder'd at, that our author first turned his thoughts to the investigation of its history.

The first chapter therefore of the work before us contains the opinions of the antients concerning coral, and the observations made thereupon since their time; among which are the opinions of Peireskius, Boyle, Piso, Boccone, Venette, the Comte de Marfigli, and those of M. de Peyssonnel.

In the second chapter is an examination, whether coral is a plant, or a congelation; in which are included two extracts, one from M. Tournefort's elements of botany, and the other from the memoirs of the Royal Academy of Sciences.

The third chapter exhibits new observations, from which are discovered the *urticæ marinæ* & *Purpuræ*, which form coral; wherein likewise are explained the formation and mechanism of this marine production.

In the fourth chapter we find new chemical observations upon the distillation of coral, which tend to prove, that coral is the production of insects.

In

In the fifth chapter are exhibited the definition, etymology, colours, and different sizes of corals, and of the insects inhabiting therein.

The sixth chapter shews us the places, where they fish for coral, and the manner of fishing for it.

In the seventh chapter we have the manner of working upon, and of polishing coral, and the commerce therewith.

The eighth, ninth, and tenth chapters give us the chemical preparations of coral, its virtues and uses in medicine, when variously prepared.

The subjects of the eight dissertations of the second part of this work are the several species of vermicular tubes found in the sea, the madrepores, millepores, lithophytons, corallines, sponges, the various shell-fish, which inhabit the sea without changing their place, and the formation and mechanism of these several substances.

This then is the general scope of our author; and though every part of his work deserves to be considered, I must, upon account of the time usually allowed to works of this nature, confine myself to such parts only, as seem most to merit the attention of the Royal Society.

It had been long the received opinion, that coral was soft in the sea, and was harden'd by the air upon taking it out of the water; and our learned Mr. Boyle was not willing to quit this opinion. But as experiments are the only way of assuring ourselves of the truth, Boccone, for this purpose, went to sea in one of the coral-fishers vessels, and by plunging his arm into the water had an opportunity of examining the coral, as they were fishing it up, before it came

into the air. He invariably found it hard, except at its extremities; where, upon pressing it between the nails of the fingers, it furnished a small quantity of a milky fluid, resembling in some degree the juice of spurge or sow-thistle. Boccone observes farther, that he saw several furrows under the bark of the coral, which terminate at the extremities of the branches, about which one might clearly see several small holes of the form of a star, which he imagines are destined for the production of branches. Vennette's account of coral in his treatise of stones is much the same as Boccone's.

The Count de Marfigli, in a letter to the Abbé Bignon, in the year 1706, takes notice, that, in order to give the most exact account of the production of coral, he wanted to be assured, whether the milky juice before-mentioned was found therein both in winter and summer, which was a matter of dispute even among the coral-fishers. For this purpose he went in winter for a few days to sea with the coral-fishers, and made several important discoveries into the nature of coral. He sent the Abbé Bignon an account of some branches of coral, which he found cover'd with flowers, and which was a thing unknown even to the coral-fishers themselves. These flowers were about a line and a half in length, supported by a white calyx, from which proceeded eight rays of the same colour. These were of the same length, and of the same distance one from the other, and formed a star-like appearance. These bodies, which the Count de Marfigli imagined were flowers, M. Peyssonnel afterwards discover'd to be the insects inhabiting the coral. As to the fact, whether the coral furnished a milky juice  
in

in winter as well as in summer, Count de Marfigli observed, that he did in December find the milky juice between the bark of coral and its substance, in the same manner as he did in the month of June preceding.

M. de Peyssonnel was unwilling, that the idea, which the ingenious discovery of the Count de Marfigli had given, in relation to the flowers of coral, should be lost; and therefore, being at Marseilles in the year 1723, he went to sea with the coral-fishers. Being well apprised of what Marfigli had observed, and the manner of his making these observations, as soon as the net, with which they bring up the coral, was near the level of the water, he plunged a glass vessel therein, into which he convey'd some branches of coral. Some hours after, he observed, that there appear'd a number of white points upon every side of this bark. These points answer'd to the holes, which pierced the bark, and formed a circumscribed figure with yellow and white rays, the center of which appear'd hollow, but afterwards expanded itself, and exhibited several rays resembling the flower of the olive-tree; and these are the flowers of coral described by Marfigli.

Having taken this coral out of the water, the flowers enter'd into the bark, and disappear'd; but being again put into the water, some hours after they were perceptible again. He thought them not so large as the Count de Marfigli mentions, scarce exceeding in diameter a large pin's head. They were soft, and their petals disappear'd, when they were touched in the water, forming irregular figures. Having put some of these flowers upon white paper,

they lost their transparency, and became red as they dried.

Our author observed, that these flowers grew from the branches in every direction, from broken ones, as well as from those which were whole; but their number lessened towards the root; and after many observations he determines, that what Marfigli took for flowers were truly insects.

Coral is equally red in the sea as out of it; and this redness is more shining, when just taken out of the water, than even when it is polish'd. The bark of coral, by being dried, becomes somewhat pale. The extremities of its branches are soft, to the length of five or six lines; they are fill'd with a whitish juice tending to yellow. The coral-fishers said, that in the month of May this juice did sometimes appear upon the surface of the bark; but this, notwithstanding great attention, our author could not observe.

The body of coral, although hard, seems to give way a little, when press'd between the fingers; and being broken at different distances, when just taken from the water, there always came therefrom a small quantity of milky juice through certain tubes, which appeared to be destin'd towards the bark.

Having inquired of the fishers in what direction the coral grew in the sea, they acquainted him, where the depth of the sea permitted them to dive, that they had found it growing sometimes perpendicularly downwards, sometimes horizontally, and sometimes upwards.

Having verified these observations during the eight days he staid with the fishermen, he adds, that he had never found any pores perceptible in the substance  
of

of the coral; that there issued forth less milk from the large branches than from the smaller ones; and that the first were harder, and less compressible.

The bark of coral covers the whole plant from the root to the extremities of the smallest branches. It will peel off; but this is only when just taken out of the water. After it has been exposed for a short time to the air, you cannot detach it from the body of the coral, without rubbing it to powder. This bark appears pierced with little holes, and these answer to small cavities upon the substance of the coral. When you take off a piece of this bark, you observe an infinite quantity of little tubes, which connect the bark to the plant, and a great number of little glands adhering to these tubes; but both one and the other do not distinctly appear, except when they are full of juice. It is from these tubes and glands that the milky juice of coral issues forth. Besides these, you see in variety of places the bark push itself outwards, where the substance of the coral is hollow'd, and form'd into the little cells, taken notice of by Boccone and Marfigli. In these you see little yellowish bodies, of the length of half a line, which terminate at the holes in the bark; and it is from these that the flowers appear.

Our author has found branches of coral, which, having been broken, have fallen upon other branches, have fasten'd themselves thereto, and have thus continued to grow. He has found, when a piece of stone, shells, or other hard bodies, have offer'd themselves between the ramifications of coral, that it has expanded itself over them, and enveloped them in its substance. He has seen pieces of coral growing  
upon



upon detached pieces of rock, glass bottles, broken pots, and other substances, from which the plant could receive no nourishment. It has been said by great authority, that coral grows from the rocks perpendicularly downwards; but our author has seen some growing to a round flint, which must necessarily have vegetated upwards, like most other plants.

M. de Peyssonnel proceeds to examine, whether or no coral is a plant, according to the general opinion, or a petrification or congelation, according to some; and after have exhibiting the various arguments deliver'd in support of these, he concludes, that coral, as well as all other stony sea-plants, and even sponges, are the work of different insects, particular to each species of these marine bodies, which labour uniformly according to their nature, and as the Supreme Being has order'd and determin'd. The coral-insect, which is here called a little *urtica*, *purpura*, or polype, and which M. Marfigli took for its flower, expands itself in water, and contracts itself in air, or when you touch it in water with your hand, or pour acid liquors to it. This is usual to fishes or insects of the vermicular kind.

When our author was upon the coasts of Barbary in 1725, he had the pleasure of seeing the coral-insect move its claws or legs; and having placed a vessel of sea-water with coral therein near the fire, these little insects expanded themselves. He increased the fire, and made the water boil, and by these means kept them in their expanded state out of the coral, as happens in boiling shell-animals, whether of land or sea. Repeating his observations upon other branches, he clearly saw, that the little holes, perceptible upon the bark of the coral, were the openings  
through

through which these insects went forth. These holes correspond with those little cavities or cells, which are partly in the bark, and partly upon the substance of the coral; and these cavities are the niches, which the insects inhabit. In the tubes, which he had perceived, are contained the organs of the animal; the glandules are the extremities of his feet, and the whole contains the liquor or milk of coral, which is the blood and juices of the animal. When he press'd this little elevation with his nails, the intestines and whole body of the insect came out mix'd together, and resembled the thick juice furnish'd by the sebaceous glands of the skin. He saw, that the animal, when it wanted to come forth from its niche, forced the sphincter at its entrance, and gave it an appearance like a star with white, yellow, or red rays. When the insect comes out of its hole without expanding itself, the feet and body of it form the white appearance, observed by Marfigli; but being come forth, and expanded, it forms what that gentleman and our author took for the petals of the flowers of coral, the calyx of this supposed flower being the body of the animal protruded from its cell. The milk before-mention'd is the blood and natural juice of the insect, and is more or less abundant in proportion to its health and vigour. When these insects are dead, they corrupt, and communicate to the water the smell of putrid fish.

The substance of coral, by a chemical analysis, scarce furnishes either oil, salt, or phlegm: live coral with its bark furnishes about a fortieth part of its weight in these; but the bark of coral alone, in which are contain'd these animals, affords a sixth  
part.

part. These principles resemble those drawn from human skull, hartshorn, and other parts of animals.

After the accounts here laid down, we are able to assign the reasons of all the particular facts we observe in coral. We see, why a branch thereof, broken off and detach'd from its stem may flourish. It is because the coral-insects, which are contain'd in its cells, not having been injured, continue their operations; and drawing no nourishment from the stem of the coral, are able to increase, detached and separate. How they live and are nourished, is proposed to be explain'd in treating of the urchin of the Madrepora, in which these animals are vastly larger, and appear very distinctly.

In each hole or star of the Madrepora, on which our author lays the evident proof of his new system, the urchin, placed in the centre of each pore, causes it to increase in every direction, by lifting itself further and further from the centre of the stone. And in coral, and in the lithophyton, the urchin, being niched in their crusts or barks, deposits a juice or liquor, which runs along the furrows perceiv'd upon the proper substance or body of coral, and, stopping by little and little, becomes fixed and hard, and is changed into stone; and this liquor, being stopped by the bark, causes the coral to increase proportionably, and in every direction. In forming coral, and other marine productions of this class, the animals labour like those of the testaceous kind, each according to his species, and their productions vary according to their several forms, magnitudes, and colours.

If, after what has been here laid down, some will still consider these marine productions as plants, they  
are

are truly zoophytes, formed by the labour of the animals, which inhabit them, and to which they are the stay and support.

By what is exhibited in this work, the author conceives, that he has explain'd the nature of these several marine productions, which have hitherto been so enigmatical. It is true indeed, that no reasons can be assign'd, why the oeconomy of these animals is directed in such or such particular forms. We can no more account for the admirable structure and colour of several species of shell-fish: we must in this, as in most of the other operations of nature, cry out, *O altitudo divitiarum!*

Swammerdam seems to have proceeded very far in these discoveries, as you may see by his letter to Bocconi\*. He goes farther, and says, that having with a microscope examin'd a piece of coral, he found, that each particle thereof was composed of ten or twelve angular and chrystalline spherules; and having saw'd across a piece of coral, and given it the highest polish, he found with the microscope, and even without it, that coral from its centre is disposed in *strata*, which he conjectures are form'd by the application of the above-mention'd spherules.

M. de Reaumur, having been made acquainted with what M. de Peyssonnel had observ'd, sent him a letter thereupon in the year 1726; wherein he takes notice, that no one had hitherto consider'd coral as the work of insects. But it seem'd to him difficult to establish this doctrine in the generality of

M m m

marine

marine productions, as was our author's opinion. That however you consider'd coral and lithophytons, it did not appear possible, that they were the constructions of the insects inhabiting therein: That the only system to be adopted upon these matters, was, what he mention'd to our author heretofore; and that is, that the bark of these bodies only is a plant properly speaking; and that this deposits a stony matter, which forms the stalk necessary to sustain it. That then, in his opinion, all the difficulties vanish with regard to the organization of coral.

In the year 1726, M. de Peyssonnel was appointed physician-botanist to the island of Guadeloupe, where he has continued his observations, which have more and more convinc'd him of the truth of his system. He takes notice, that the leprosy, a disorder elsewhere almost unknown, is frequent in this island: Our author must mean here the *elephantiasis*, or leprosy of the Greeks; as that of the Arabians is too frequent every-where.

M. de Peyssonnel acquaints us, he has since found, that M. Bernard de Jussien and M. de Reaumur were themselves convinc'd of the truth of his observations in the expeditions they have made, one to the sea-coasts near Rochelle, and the other in Normandy.

In the course of this work our author mentions, that, besides the animals to which coral owes its formation, there are three kinds, which he describes at large, which pierce and corrode the coral while in the sea, without preventing its increase.

Contrary to what has been generally received, and to what even the Count de Marsigli asserts, coral  
grows

grows among the rocks, and in the caverns of the sea, open to every exposure. It had always been said, that it never grew in caverns open to the north; they must always be exposed to the south, at least to the east or west: but upon the coast of Barbary, which lies open to the north, coral is not less frequently found than elsewhere. It is generally observed to grow better and more readily in shallow than in deep water; and though they generally fish for it at the depth of ten or twelve fathom, they sometimes get it, though but seldom, at an hundred and twenty.

M. de Peyssonnel then gives us the manner of coral-fishing, and describes two different machines made use of for this purpose: one, for fishing up the coral where the bottom is smooth, and it is the same, which is described by Gassendi in his life of Peyreskius. The other, which is called in the Provençal language the *salabre*, is constructed so, as to be employ'd where the bottom of the sea is rocky and unequal. He takes notice of the great skill and address of the coral-fishers in the management of these machines, as well as their sagacity, in finding, at considerable distances from the shore, the very places, where some time before they have been successful. I am sorry he has omitted to send us the figures and representations of this fishing, which he tells us he has in his museum.

In the course of this work, our author takes notice, that all the productions of the sea, of which he is now treating, have been consider'd by naturalists sometimes as stones, and sometimes as plants. Their stony substance deceived some, their tree-like appearance

others; infomuch that the bulk of writers, who have not seen these bodies except in their cabinets, have only consider'd their figures. They have denominated pora that class of them, which seem'd pierc'd with holes. Of these they found some, the holes of which were large; and these they call'd madrepora.

There is another confusion among the writers concerning these bodies: all those, which had a tree-like form, whether their surfaces were smooth, without holes, or whether they were rough and unequal with them, they were all together stiled corals. Those of any other form than that just now mentioned, were call'd madrepora, lithophyton, or alcyonium. It therefore appears necessary to establish some essential characters to be able to distinguish these different bodies one from another; but before these marks of distinction are laid down, our author thinks proper to examine, what these bodies are, and how they are formed. He proceeds to remark, that divers productions are found in the sea of a stony nature. These bodies are always equal, and always the same in their different species: they have the same arrangement of parts, the same essential figure, and differ in nothing but in their outward form, like different vegetables. They are all pierced with holes and pores, which are of the same size and figure, and are of the same disposition in each species; so that it appears evident, that they are all produced from the same matter. How they are produced, and their mechanism, has been hitherto unknown.

Our author has given us, when treating of coral, several observations of other persons relating thereto ; but he finds none relating to the madrepora, and the other sea productions. But the knowledge, which he had acquired into the nature of coral, conducted him to the discovery, which he made, of the animals, that form the madrepora.

As this system is new, he thinks it necessary to give his observations, as they enabled him to form it. He defines the madrepora's to be all those marine bodies, which are of a stony substance, without either bark or crust, and which have but one apparent opening at each extremity, furnish'd with rays, which proceed from the centre to the circumference. He then takes notice of the means, by which he found the madrepora to be the habitation of animals. So early as the year 1719, when his curiosity carried him to the coral-fishing on the coast of Provence ; and though intent only upon coral, and neglecting to examine any other marine production, he nevertheless observ'd, that the extremities of the madrepora were soft, and cover'd with a mucosity, which had a fishy smell. From thence he suspected, that therein was contain'd some kind of animal, but his curiosity stopp'd here. Afterwards, being upon the coasts of Barbary, the fishermen brought him in a barrel of sea-water one of those madrepora's, which are call'd in Provence, *fenouille de mer*, or sea-fennel. It had been put into the barrel as soon as it was taken out of the sea ; and he observ'd, that the extremities of this madrepora were soft and tender, furnish'd with a transparent mucosity, like that of  
snails :



nails; these extremities were of a beautiful yellow colour, and were five or six lines in diameter.

He therein saw an animal, resembling the cuttle-fish, polype, or sea-nettle. The body of this fish fill'd the centre; its head was placed in the middle thereof, and was surrounded by several feet or claws: these feet fill'd the intervals of the partitions observ'd in the madrepora, and were at pleasure brought to its head, and were furnish'd with yellow *papillæ*. Its head or centre was lifted up occasionally above the surface, and often contracted and dilated itself like the pupil of the eye. He had the pleasure of seeing it move distinctly all its claws, as well as its head or centre.

We can easily conceive all these motions, from what we all of us have lately seen in the fresh-water polype, discover'd by our worthy member Mr. Trembley: and it is to be observ'd, that the great sea polype (which is eaten in Lent in the Mediterranean, and which is found upon our own coasts, and usually call'd a *prule*) the animal of the madrepora, that of coral, and the fresh-water polype, scarce differ but in magnitude; so that from having seen one, an idea of the rest may easily be formed. And I mention this with the more freedom, as I myself, upon a visit with Mr. Trembley in Sussex at the late excellent Duke of Richmond's, whose loss we yet lament, saw the same order and oeconomy observed in the coralline\*, as is mention'd by M. de Peyssonnel of the

\* In that species of it intituled by Mr. Ray, *Corallina minus ramosa alterna vice denticulata*. Vide Raii Synopf. Edit. 3. p. 35.

the coral and madrepora. This phenomenon Mr. Trembley had discover'd some time before ; and having put some fresh collected coralline into a phial of sea-water, brought it to Goodwood ; where, after it had been suffer'd to remain at rest a few hours, by the assistance of a microscope a great number of very small white polypes, exactly in form resembling the fresh water polype, but infinitely less, were seen to protrude themselves from the inequalities of the coralline, each of which serv'd as an habitation for a polype. When the water was still, these animals came forth, and mov'd their claws in search of their prey in various directions ; but, upon the least motion of the glass, they instantly disappear'd ; as was the case of the coral-insect, describ'd by our author.

But to return. The flesh of the animal of the madrepora is so soft, that it divides upon the gentlest touch. This soft texture prevented M. de Peyssonnel from detaching any one ; and he observes, that there are in those seas several large species of urtica, which become pappy upon the least touch. He mentions one sort of above a foot in diameter, whose body is as large as a man's head, and which are of a poisonous nature.

After the madrepora had been preserved three days, the animals therein cover'd its whole surface with a transparent jelly, which melted away, and fell to the bottom of the water as the animal died ; and both the water and madrepora then had a putrid fishy smell. After having destroy'd and consum'd all the animals, the extremities of the madrepora became white.

*Imperatus*

Imperatus seems to have border'd upon this discovery, when he says, " that the extremities of the  
 " madrepora are soft, of an obscure purplish colour,  
 " containing a membranous substance ; from whence  
 " one might suspect, that it partakes of a sensitive  
 " and animal life."

Our author made the experiment here laid down upon every species of madrepora, which he found, during the three months he continu'd upon the coasts of Barbary. He observ'd always the same appearance, allowing some little difference for the colour and size of the animals, the texture of their bodies, and that of the bodies themselves, upon which they were produced.

From what I have here extracted concerning the coral and madrepora, an idea may be formed of the millepora, lithophyton, corallines, and sponges ; each of which is, according to our author, the habitation of numerous animals, and form'd by them. He has given us from his own observations particular accounts of each of these productions, and divided them into *genera* and *species* with great accuracy ; and though in common they are the habitations of animals, each species varying in form and bulk, and composing its cell in various forms and manners, and of different consistences, constitutes their essential character. As oysters, scallops, muscles, cockles, snails, &c. have a power given them by the Author of nature of forming and enlarging their separate dwellings ; to these bodies, the subjects of this treatise, the same power is given, but in large families.

In the madrepora, its animal occupies the extremity ; in the millepora, the substance ; in corallines  
 and

and sponges, the void places; in coral and lithophytions, the cortical parts.

Each of these animals, according to their kind, furnish substances, differing, as much in consistence as in form. That of coral is extremely hard, and compact; the madrepora and millepora are of a stony, but more loose texture; the coralline is still more soft; the lithophyton of a substance nearer horn than stone; and the sponge is soft and elastic.

We observe a great variety in the operations of nature: the crab, the cuttle-fish, and the sea spider, are endow'd with a testaceous covering; the esculent sea polype, and others of that class, have no such defence. So most of the animals, hitherto taken notice of in this treatise, have a secure retreat; but there is a production, denominated by Imperatus *Lorica marina*, which has no such convenience. It is, if I may be allow'd the expression, a soft madrepora. It grows at the bottom of the sea, and is a series of circular tubes, of about half an inch long, and of two or three lines in diameter. Each of these, at the end most remote from the centre, is furnish'd with a sphincter, from which are occasionally protruded the legs or claws of the animal, like those before-mention'd. The tubes themselves are likewise at pleasure lengthen'd and shorten'd. They are fasten'd to the rocks by a common broad surface, after the manner of coral, and such-like marine productions, and are of a coriaceous substance. Hither likewise may be referr'd the soft lithophyton, usually call'd the sea mulberry, and described by our author,

which, upon observation, exhibits nearly the same phenomena with the preceding.

It remains now, that I lay before you some account of our author's opinion concerning the propagation of these animals. He supposes, that they spawn, as oysters do ; and that their spawn is enveloped in a viscous substance, like that of testaceous and other fish ; and that by this viscosity it is fasten'd indifferently to whatever solid body falls in its way, whether it be a rock, glass, broken pots, flint-stones, and such-like. This viscous matter, coming to stagnate, is changed, according to its nature, into a solid, and forms a *lamina* or *stratum*, such as is observed at the base of these productions, and serves as it were for their first principle. The egg, enveloped in this viscous substance, is hatch'd in its proper time, and furnishes the animal, which resembles the sea polype, and other soft fish. These animals have all the necessary organs, and among others a particular gut, which, in the cuttle-fish, is fill'd with a black liquor, the use of which, according to the vulgar opinion, is that of being pour'd out at pleasure, to prevent the animal being taken when pursued : but this liquor, according to our author, serves the animals, the subjects of this treatise, with a matter capable of growing hard ; and furnishes the increase of the body or shell of the animal, which, like other shells, remains always of the same form, and is of a size proportionable to the animal. In the madrepora it lifts itself up under the animal, which always lies upon it ; but in the millepora it increases from the centre as the animal grows larger ; and thus these marine productions grow in just proportions.

**These**

These animals are .nourish'd without changing their place, like American oysters, which fasten themselves to the roots of the *mangles*; or like what has been heretofore call'd *concha anatifera*, which fastens itself to old planks. Nature has furnish'd these polypes with claws, which they occasionally protrude from their cells, and seize their prey, as it passes by them; and thus they are nourished, and increase, according to their particular mechanism and construction.

There are some species of the polype of the madrepora, which are produced singly, others in clusters. The first of these kinds may arise from the parent animal furnishing but one egg at a time: other species deposit a number of these eggs at the same time, which coming to life all together are joined in such a manner, that they seem to constitute one and the same body.

The millepora's grow one upon another; their little animals produce their spawn, which attaching itself either to the extremity of the body already formed, or underneath it, gives a different form to this production. Hence the various shapes of the millepora, which is composed of an infinite number of the cells of these little insects, which all together exhibit different figures, notwithstanding that every particular cellule has its essential form, and the same dimensions, according to its own species.

I have thus endeavour'd, in as concise a manner as I was able, to communicate some account of the labours of the very ingenious author of the work before us. The time allow'd by the Society for these extracts does not permit me to give any idea of his

arrangement of the great variety of bodies, the subjects of this treatise, which is interspersed everywhere with very curious remarks. You see, that M. de Peyssonnel, if his system is admitted, has made a great alteration in that part of natural history, of which we are now treating. Naturalists had been divided, whether coral, and the harder productions of the sea, should be consider'd as plants or stones. Those, who look'd upon them as stones, among whom was Dr. Woodward, imagin'd themselves justified in this opinion, from their excessive hardness, and from their specific gravity; and they were herein confirm'd, by observing, that if these bodies were calcined, they were converted into lime. Guisónæus, in his letter to Boccone, says positively, that coral is not a plant, but a real mineral, composed of much salt, and a small quantity of earth: he supposes its form given it by a precipitation, something like that of the *arbor Diane* of the chemists.

Dioscorides, Pliny, Cæsalpinus, Boccone, Ray, Tournefort, and Geoffroy, thought coral to be a plant, from its root's being fixed to rocks or stones, as those of trees are to the earth; and from its sending forth a trunk, which ramified into branches. This opinion was seemingly strengthen'd by Boccone's observation of the milky juice at the tops and in the cells of coral; and most of all by the Count de Marfigli's discovering, in the year 1706, what he conjectured were the flowers of coral. Both these opinions, countenanced by long time, and great authority, M. de Peyssonnel has endeavoured to overturn; and to shew, that these productions were neither stones, nor vegetables, but animals; and that,  
like

like oysters, and other shell-fish, nature had impower'd them to form themselves a stony dwelling for their protection and support, each according to its kind.

Some account of M. de Peyssonnel's discoveries was transmitted by him to the Royal Academy of Sciences at Paris in the year 1727; but they were not much attended to, till our ingenious brother Mr. Trembley's discovery of the fresh-water polype. This added much to their weight, and occasion'd M. Bernard de Jussieu, of this Society, and of the Royal Academy of Sciences at Paris, to visit, in the year 1741, the sea-coasts of Normandy, in order to satisfy himself of the nature of these marine productions; and his observations confirmed those of M. de Peyssonnel. The sentiments of that great naturalist M. de Reaumur upon this subject may be seen at large in the preface to the sixth volume of his history of insects.

I cannot conclude this account, without observing, that, in my opinion, the Royal Society is greatly obliged to M. de Peyssonnel, for his transmitting this manuscript, which I consider as a very valuable literary present.



LXXVI. *A Letter from Mr. Rich. Brooke, Surgeon, to James Parsons, M.D. Secretary to the Royal Society for foreign Correspondence, concerning Inoculation.*

S I R,

Read May 14,  
1752.

**A**S there are very few, who escape having the small-pox sooner or later in life, and as very terrible consequences too often attend the being seized with it in the natural way, it is no wonder, that most people, who have not yet had it, live in continual apprehensions and fear thereof; or that the great and evident advantages of inoculating young persons should have so universally recommended, and so firmly establish'd, that practice, which probably will never be laid aside, till some easier and equally certain method be discover'd.

Though such a discovery may, at first, seem a thing rather to be wish'd than hoped for, yet I flatter myself, that an accident, which happen'd some years ago in my practice, and the experiments, which I have since made in consequence of the hint thereby given me, may, in some measure, contribute, if not lead directly to it.

In the year 1747, I inoculated a young gentleman in Maryland, then about twenty years of age. I made a slight incision, about an inch long, on the belly of the *biceps* muscle. In that I laid the lint impregnated with variolous matter, cover'd with a digestive pledgit; then bound them on with a roller.

When

When I went afterwards to look at his arm (the roller being too slack) I found the pledgit and lint were moved to the opposite side from the wound: the incision itself was but a little discolour'd, but the part, whereon the lint lay, after its removal, was inflamed, and full of red pimples. I was afraid, that the gentleman would not be affected with the disorder; but we were not more successful than I expected; for he had the fever, eruptions, &c. at the usual times.

As he had but thirty odd pustules in all, he went thro' the different stages of the disorder without the least threatening symptom.

This induced me to try to communicate the disorder, without making any incision; that is, by applying the infected lint to the arm, and confining it with an adhesive plaister. The few patients, whom I tried this method on, were children, and always with success. The absorbent vessels, I believe, in young subjects especially, will always take in a sufficient quantity of the matter to contaminate the whole mass of the circulating fluids; and tho' the density of the pores, or scaly inspissations of the *materia perspirabilis*, in adults, may, in some measure, prevent the disorder from being communicated by contact; yet friction, as you, sir, very justly observed to me, when I mention'd it to you, will easily remove that obstacle; for by this means we may make the cuticle as thin as we please, and the warmth induced by friction will dilate the mouths of the absorbent vessels, and draw a moderate flux of juices to the part,

so that they may take in a sufficient quantity of variolous matter to bring on the disorder. I am, Sir,

May 2, 1752. Your most obedient humble servant,

R. Brooke.

\* Since the above account was communicated to the Royal Society by Mr. Brooke, the experiment has been tried upon four children by Dr. Conyers at the Foundling-Hospital, but was followed neither by the variolous fever or eruption in any one of the instances.

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LXXVII. *A Sequel of the Case of the Right Honourable Horace Walpole, Esq; relating to the Stone, since his first Account in April 1750* \*.

Read May 28,  
1752.

**A**FTER having found myself for two years together perfectly well, and free from all symptoms of my former disorder, having taken for some time no more than one half of the quantity of soap and lime-water that I had before used; in November 1750, I came out of the country in my coach in the usual travelling pace, without the least inconvenience. But having ventur'd sometimes to go in a coach, after I came to town, upon the stones, I began at times to feel the symptoms of the same disorder, which upon any motion, besides that of going in a chair, even in walking to any degree, increased upon me; and driving only in my chariot thro' the two parks to Kensington, without going upon the stones, I found myself greatly affected, by making frequently and involuntarily water, and sometimes bloody, accompanied with sudden

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\* See above, p. 43.

den stops, and severe pains. However, taking the precaution of going by water as far as the Old Swan, and being carried from thence in a chair as far as Whitechapel, I ventur'd in a chariot, fitted up with the best French springs, to go into the country with Mrs. Walpole about midsummer last: but before I had got half way to Epping, tho' the horses went but a gentle pace, I felt as great uneasiness, attended with the same severe symptoms, as I had ever done; which frequently returned, and continued upon me during the whole journey for four days together, with little or no abatement, except while I was in bed; whereas formerly, after I had lain some time, I was perfectly easy the whole night. In lighting from my coach, upon my arrival at my house in the country, I had indeed a cruel fit; but after I had rested one night, and kept myself as quiet as possible for several days, I found myself perfectly well again; and as I never went in a coach, and did not walk much, during my whole stay in the country last year for about five months together, I never felt the least symptom of uneasiness.

A few days before I left the country in November last, I took a turn or two round my park in my chariot, free from pain; which encouraged me to undertake a journey to town again in my chariot, by short stages, and gentle driving; and it was perform'd in five days to Whitechapel, without my being sensible of the least inconvenience any part of the way; neither have I felt any since my arrival in town; and I continue well, taking daily, as I have constantly done from the time I went last into the country, the full quantity of soap and lime-water, as formerly I took.

LXXVIII. *Part of a Letter from Mr. John Parker, an English Painter at Rome, to his Father at London, concerning the late Eruption of Mount Vesuvius: Communicated by Mr. Henry Baker, F. R. S.*

Honoured Sir,

Rome, Dec. 20, 1751.

Read May 28, 1752. **I**HAD the satisfaction, whilst at Naples, to see the eruption of Mount Vesuvius, which was very extraordinary; but the want of room here hinders me the giving you any very particular account. It lasted about 25 days in all, and broke out of the side of the mountain; preceded by an earthquake, felt all over Naples at the time of the eruption. The mountain in the middle of the crater or cup, which formerly threw out the stones, sunk down, with about a third of the bottom of the said cup. The breadth of the matter it threw out is in some places half a mile over, in almost the least part 60 feet; and has filled a valley, into which it ran, that might be about 60 feet deep, and raised a mountain in the same place, of matter and ashes, about 50 feet high; and its whole length, from the mouth to where it stopp'd, is about 5 miles; but it did not arrive at the sea by near five miles. The matter, which is here called lava, seems to be composed of iron, antimony, sulphur, and salts, and is not always of the same colour, taste, &c. in every place. The thing I can compare it to most, is the large cinders thrown out of your great iron works, but cover'd over in many places with the above salts and sulphur. Whilst the lava run red-hot, I saw a man throw a  
mass

mass of the cool lava from an height upon it, which, far from sinking into it, rebounded like a ball. Its motion was as slow as the common walk of a man. It broke out in five different places. I walk'd on it for about a mile, whilst near three feet of the top were cool'd; but for many feet underneath as red to the sight as the furnace of a glass-house. It cover'd and burnt up trees, houses, &c. in short all it found in its way. From,

S I R,

Your dutiful son,

John Parker.

**LXXIX.** *The Case of a Piece of Bone, together with a Stone in the Bladder, successfully extracted by Mr. Joseph Warner, F. R. S. and Surgeon to Guy's Hospital.*

Read May 28, 1752. **T**HE stone in the bladder is a disease so common to both sexes, and the symptoms, and circumstances attending it are in general so well known, and so much alike, as to render few cases of this kind worthy of communication. But as the following is attended with a singular, and perhaps unparallel'd circumstance, I make bold humbly to offer to your consideration a short account of the following fact:

Elizabeth England, aged 48 (in all other respects an healthy woman) had been afflicted with the symptoms of the stone in the bladder for about two years, for which she put herself under my care.

After having prepared her in the usual manner, I proceeded to the operation; but in a method somewhat different from that generally practised, which is effected merely by a forcible dilatation, and consequent laceration, of the *urethra*. For having almost always observed an incontinence of urine, in consequence of this method of operating, for this reason, and from the success which I had some time ago met with, in an extraordinary case communicated to this Society, I departed from the usual method of operating, and cut the *urethra* obliquely upwards on the right side, to about half its length; which I easily did, by introducing a small knife into the groove of the staff, and found very little force requisite to the introduction of the necessary instruments into the bladder, and in the extraction of the stone, &c.

Upon laying hold of the stone, it broke; so that only a part of it, about the size of a pigeon's egg, was extracted, upon the first introduction of the forceps. Upon introducing the forceps a second time, I extracted a ragged and irregular piece of bone, weighing 16 grains, which is now submitted to your inspection. Before it was cleansed, its cavities appear'd fill'd and cover'd with a mixture of hairy and stony particles; from whence I conjecture, that it probably was the nucleus of the stone.

Nothing remarkable occur'd during the cure, but that the patient, ever since the second day after the

the

the operation, has been capable of retaining her urine, and is now perfectly well.

The operation was performed on the 7th instant.

Hatton Garden, May 28,  
1752.

**LXXX.** *An Account of a Water-Spout, rais'd off the Land, in Deeping-Fen, Lincolnshire; by the Rev. Mr. Benjamin Ray, of Cowbit near Spalding in that County; communicated to the Society at Spalding, on the 7 of May 1752, by Maurice Johnson, Esq; and by him to the Royal Society.*

Read May 28, <sup>1752.</sup> **I**N the year 1752, on the 5 of May, a very uncommon phenomenon appear'd about 7 in the evening, in Deeping-Fen, which, from its effects, I take to be a water-spout, broken from the clouds; nor can it admit, in my opinion, of any other solution.

A watry substance, as it seem'd to me, was seen moving upon the surface of the earth and water, in Deeping-Fen. It march'd along with such violence and rapidity, that it carried every thing before it; such as grass, straw, and stubble; and in its going over the country bank, it rais'd the dust to a great height; and when it arriv'd in the wash, in the midst of the water, and just over against where I live, then it was, that I first saw it; and here it was, that it stood still for some minutes. How dreadful  
was



was it to behold this moving phænomenon, now fix'd ! to see, as it were, a law of nature inverted ! for this watry substance spouted out water from its own surface, to a considerable height, and all the time attended with a terrible noise, so as all the beasts and sheep ran from it, greatly frightened.

Upon its second route, it proceeded in a side-line into the river, breaking in its passage a fishing-net, and there it moved along, till it came to the church, where it stood again some little while, and then made its next passage thro' the space, that is between the church and the parsonage-house, towards Weston hills and Moulton chapel. In its way to these places, it tore up a field of turnips, broke a gate off the hinges, and another gate it broke to pieces. Those, who saw it evaporate, affirm it ascended into the clouds in a long spearing vapour, and at last ended in a fiery stream. There was a mist, like smoke, frequently round it. Three more were seen at the same time in different places.

*P. S.* When this was read to the Society at Spalding, several of the members present attested, that they themselves saw this phænomenon in most circumstances the same as here described ; excepting only the last of a fiery stream.

But to some other people, who gave accounts of it to them, it did so appear.

LXXXI. *A Description of Two Methods, by which the Irregularity of the Motion of a Clock, arising from the Influence of Heat and Cold upon the Rod of the Pendulum, may be prevented; by John Ellicott, F. R. S.*

Read June 4, 1752. **T**HE first of these methods consists in a particular construction of the pendulum itself, which occur'd to me several years ago. In the beginning of the year 1738, I put into the hands of Mr. Machin, then one of the secretaries, a description and a drawing of such a pendulum, in order to their being laid before this honourable Society: but Mr. Machin, soon after, acquainting me, that a gentleman, of whose skill and judgment in mechanical contrivances I had always entertain'd the highest opinion, made some objections, I was advis'd to defer communicating my invention to this Society, till I should have examined into the weight of those objections, and, by a fair and impartial trial, should be fully assured, that the contrivance would answer the end propos'd. And having now at length obtain'd that satisfaction, I beg leave to give a short narrative of some of the most remarkable observations I have made during this inquiry, which I hope will not prove unacceptable to this honourable Society.

About the year 1732, an experiment, which I made, in order to satisfy some gentlemen, that the rod of a pendulum was liable to be considerably influenced by moderate degrees of heat and cold, led me.

me to consider, that, as metals differ from each other in their density, it was highly probable they might likewise differ from each other in their expansion; and that this difference of the expansions of two metals might be so applied, as in a great measure to remove those irregularities in the motion of a clock, which arise from the effect of heat and cold upon the length of a pendulum. With this view, not long afterwards I contrived the pendulum now described by Fig. 1.

In which *ab* represents a bar of brass, made quite fast at the upper part by pins, and held contiguous, at several equal distances, by the screws 1, 2, 3, &c. to the rod of the pendulum, which is a bar of iron; and so far as the brass bar reaches, is filed of the same size and shape, and consequently does not appear in the figure; but a little below the end of the brass bar, the iron is left broader, as at *dd*, for the convenience of fixing the work to it, and is made of a sufficient length to pass quite thro' the ball of the pendulum to *c*. The holes, 1, 2, &c. in the brass, thro' which the shanks of the screws pass into the iron rod of the pendulum, are filed as in the drawing, of a length sufficient to suffer the brass to contract and dilate freely by heat and cold under the heads of the screws. *eeee* represents the ball of the pendulum: *ff*, two strong pieces of steel, or levers, whose inner centres, or pivots, turn in two holes drilled in the broad part of the pendulum-rod, and their outer ones in a strong bridge, or cock, screw'd upon the same part of the rod, but omitted in the draught; because, when put on, it covers this mechanism. *gg*, are two screws entering at the edge,  
and

and reaching into the cavity near the centre of the ball. The ends of these screws next the centre are turn'd into the form represented in the drawing, which, pressing with the weight of the ball against the longer arms of the levers, cause the shorter arms to press against the end of the brass bar at *c*. Things being in this situation, let us suppose, that the rod of the pendulum, and the brass annexed to it, grow longer by heat; and that the brass lengthens more than the iron of the same length: then the brass, by its excess of dilatation, will press the short ends of the levers downwards at *c*, and at the same time necessarily lift up the ball, which rests upon the long ends of the same levers at *ff*, to any proportion necessary: And provided the ends of the screws do press upon the levers at a proper distance from the centres, the said ball will be always kept at the point of suspension, notwithstanding any alteration the rod of the pendulum may be liable to from heat or cold. What this distance ought to be, may very nearly be determined, if the difference of the expansion between the brass and iron bars is known; for the proportion the shorter arms of the levers ought to bear to the longer ones will always be, as the excess of the expansion of the brass is to the whole expansion of the iron, as may be thus easily demonstrated. Fig. 2.

Let the line *ab*, drawn perpendicular to the line *ef*, represent a bar of iron; the line *cd* a bar of brass, the pricked line *bg*, the expansion of the brass bar by the same degree of heat: let the line *gi* be drawn parallel to the line *ef*, then will *ib* represent the difference of the expansion of the two metals: thro' the points *b, g*, draw a right line  
P P P
cutting

cutting the line  $ef$ , as in  $k$ ; this line may be supposed to represent one of the levers turning upon its centre at  $g$ ,  $b$  the point where the brass bar acts upon the shorter end of the lever, and  $k$  the point where the screw acts upon the longer end of the lever, which being the place where it intersects the line  $ef$ , it is evident the ball of the pendulum will be as much raised by the lever, as it would have been depressed by the expansion of the iron; but the triangle  $ibg$  is similar to the triangle  $b g k$ ; and therefore, as  $ib$ , the excess of the expansion of the brass, is to  $bg$ , the whole expansion of the iron, so will  $bg$ , the shorter arm of the lever, be to  $gk$ , the longer arm of the lever. Q. e. d.

At Fig. 1. is placed a strong double spring, whose ends pressing against the under edge of the ball, hinder it from bending the brass bar by its forcible action thereon at the point  $b$ , which, when the ball is of a considerable weight, it might otherwise be very liable to do.

The description here given is exactly agreeable to the original contrivance; and the only alteration I have since made in it, consists in placing the screws  $gg$  within the ball of the pendulum, as represented in Fig. 4.

But as the success of this contrivance depended intirely upon the supposition, that metals were expanded differently by the same degree of heat, before I attempted to put it in execution, I thought proper to inquire what experiments had already been made upon this subject, when Mr. John Eames, a late very worthy member of this Society, put into my hands Mr. Graham's account of his quicksilver pendulum

pendulum (as it is now commonly called) published in the *Philosophical Transactions*, N<sup>o</sup> 392, which account I found to be introduced by the following paragraph :

“ Whereas several, who have been curious in  
 “ measuring of time, have taken notice, that the vi-  
 “ brations of a pendulum are slower in summer than  
 “ in winter ; and have very justly supposed this al-  
 “ teration has proceeded from a change of length in  
 “ the pendulum itself, by the influences of heat and  
 “ cold upon it, in the different seasons of the year ;  
 “ with a view therefore of correcting, in some de-  
 “ gree, this defect of the pendulum, I made sever-  
 “ al trials, about the year 1715, to discover whe-  
 “ ther there was any considerable difference of ex-  
 “ pansion between brass, steel, iron, copper, silver,  
 “ &c. when exposed to the same degrees of heat,  
 “ as nearly as I could determine ; conceiving it would  
 “ not be very difficult, by making use of two sorts  
 “ of metals differing considerably in their degrees of  
 “ expansion and contraction, to remedy, in great  
 “ measure, the irregularities, to which common pen-  
 “ dulumns are subject. But although it is easily dis-  
 “ coverable, that all these metals suffer a sensible  
 “ alteration of their dimension by heat and cold ;  
 “ yet I found their differences in quantity, from one  
 “ another, were so small, as gave me no hopes of  
 “ succeeding this way, and made me leave off pro-  
 “ secuting this affair any farther at that time.”

The reading this paragraph proved at that time sufficient to make me lay aside all thoughts of succeeding in a contrivance founded upon principles, which a gentleman of so great abilities, and known

accuracy in making experiments, had, after trial, judged to be insufficient. And it was not till about the latter end of the year 1734, that I again resumed them on the following occasion. A gentleman desirous to make some experiments concerning the expansion of metals, employ'd me to make him an instrument like one invented by Mr. Muschenbroek for that purpose, which he calls a pyrometer. Upon looking over Mr. Muschenbroek's experiments, I not only found the difference between the expansion of some of the metals much greater than I expected, but, as I thought (if they were to be depended upon) sufficient to answer my former purpose. This led me to consider the structure of the instrument, which Mr. Muschenbroek made use of in his trials, and upon examination, I thought it liable to some objections, which I imagined would make the result of experiments made by his instrument very uncertain. I therefore endeavour'd to contrive one of a different construction, that might be more to be depended upon. Such an instrument I some time afterwards completed, and had the pleasure to find it so far met with the approbation of several very worthy members of this Society, that, at their particular desire, I drew up a description of it, which was read, and the instrument itself shewn to the Society on the 8 of April 1736\*: and though it was not in every respect

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\* This appears from the minutes of the Royal Society; tho' the description and manner of that instrument was, by some mistake, placed six months later in the *Philos. Transf. Vol. XXXIX. N. 443.* for October 1736. The other dates, mention'd in this paper, relating to that instrument, are likewise justified by authentic vouchers, which have been produced by Mr. Ellicott.

spect so accurate as I could wish, I am fully persuaded, that such experiments, as are carefully made with it, may be depended upon, as very near the truth. Having made a great variety of experiments with this instrument upon bars of different metals, as nearly of the same dimensions as possible, I found, upon a medium, their several expansions by the same degree of heat to be as follows :

Gold	Silver	Brass	Copper	Iron	Steel	Lead
73	103	95	89	60	56	149

And as I found so great a difference between the expansion of brass and iron, I immediately determined to make a pendulum after the manner above described, composed of those two metals, and likewise order'd a clock to be made, with the utmost care and exactness; and, as I then apprehended, with some considerable improvements, with which I intended to make the experiments. These were both finished in the beginning of the year 1738; and having no reason to doubt of success, I shew'd the pendulum to the late Mr. Machin, and gave him a drawing and description of it, in order to its being communicated to the Royal Society; but, as I have already observed, objections were made to it, of which the only one, that appeared to have any weight, was, that it had been found by experiment, that two bars of different metals, screw'd together, so as to be in contact with each other, would not expand regularly and smoothly, but by jerks. In order to examine into the force of this objection, I directed two bars of equal dimensions to be made, one of brass, the other of iron, of about two feet in length, fasten'd together after the same manner, as the two rods of the pendulum,



pendulum, which I intended to place so, that, by acting very near the centre of an index of a considerable length, even the smallest alteration in the bars would be made sensible, and by the motion of the index I should be able to form a judgment, whether the rods moved regularly and free'y, or not : but before this was put into execution, I contrived, by fastening the two bars to the back plate of a clock, not only to make them answer the end above propos'd, but, at the same time, to lengthen or shorten a pendulum of a common construction, in such a manner, as sufficiently to correct the irregularities arising from the influence of cold or heat upon it. The manner of applying them is described by Fig. 3.

In which, *a a a a* represents the back plate of the clock, *b b b*, a triangular piece of brass, screw'd by two screws, thro' the slits *c c*, to the plate, yet so that it may be drawn backwards or forwards by means of the screw at *d*. *ef* is a brass bar, about two feet in length, made fast at the bottom, by a screw and two pins at *f*, to an iron one of equal dimensions, to which it is likewise screw'd by the screws 1, 2, 3, &c. after the same manner as the rod of the pendulum already described. The iron bar is fastened at the upper end to the triangular piece of brass, nearly under that part of the brass bar marked *e*. *gb* is a strong brass or iron lever, moveable upon a centre at *g*, and is supported by the upper end of the brass bar ; *ii* is the cock, on which, in a common clock, the pendulum is hung ; *kk*, part of the rod of the pendulum, whose spring passing thro' a fine slit in the cock *ii*, is fasten'd to a stud riveted into the lever at *l*. The slit in the cock  
must

must be made so close, as to prevent the spring from having any lateral motion in it.

From this description it is evident, that, if the brass bar expands more than the iron one, it will raise up the lever, and, consequently, the pendulum, which is fasten'd to it; and, as the length of the pendulum is only from the centre of oscillation to the under part of the slit, thro' which the spring passes, the pendulum will be thereby shortened; and, by making the point of the brass bar to act upon a proper part of the lever (to which it is capable of being adjusted by means of the screw *d*) the pendulum may be shortened to whatever degree shall be necessary.

To prevent the pendulum from bending the bars, which it would be liable to do, if the ball of the pendulum was of any considerable weight, the end of the lever, farthest from its centre of motion, is hook'd to the end of a chain, which is wound about and fastened to a small pulley at *m*. Upon the same arbor, to which this pulley is fixed, is fastened another pulley, of a much larger diameter, to which is hung, by a silk line, the weight or counterpoise *n*. By means of this counterpoise, any part of the weight of the pendulum, that shall be desired, may be taken off from pressing against the brass bar. And if, upon the end of the arbor, to which the pulleys are fixed, an index be placed, so as to point to a graduated circle, the least motion of the lever will not only be easily perceived, but also whether that motion is uniform and regular, or not. And upon having, some time after, made a clock with this contrivance added to it, I had the pleasure to find the index not only to move very sensibly, but very regularly, and never,  
that

that I could perceive, by jerks. And I doubt not, but, when the point of bearing of the brafs bar upon the lever is once well adjusted, it will be found to lengthen or shorten the pendulum to as great a degree of exactness, as any other method whatsoever. But, as I have not as yet thought of any other method of adjusting it, except from actual trial in different seasons of the year, I must prefer the pendulum to this method, which, from the great ease, as well as exactness, with which it is capable of being adjusted, will, I think, appear to have much the advantage over any other contrivance yet made use of for this purpose.

The method I take for adjusting the longer arms of the levers of the pendulum to the shorter ones is described in Fig. 4. To a strong post, fixed to the wall, is fastened a small shelf, supported by two brackets *a b*. In the middle of this shelf is fastened a wire, by the screw *e*; to the end of which the pendulum is to be hung. Below this shelf, at the distance of about 40 inches, is placed the index *c d*, turning freely upon a center: The length of the index is 50 inches. At the distance of half an inch, upon a part of the index produced beyond the centre, is placed a steel pin; and in the back of the pendulum, as near the centre of oscillation as may be, is drilled an hole to receive this pin; when the pendulum is hung upon the wire against the post, and the wire is screw'd higher or lower by the screw *e*, till the pin resting against the upper part of the hole (which is filed into a proper shape for that purpose) keeps the index nearly in an horizontal position. Below the bottom of the pendulum is placed a second  
index

index *f'g*, exactly like the former, except that it is kept in an horizontal position, by the screw *k* bearing against the end of the iron rod. When the experiment is to be made, the pendulum is first put into a box, and gradually heated by a large fire, to a considerable degree, being often turned, that every part may be equally exposed to the fire. And having continued shut up in the box for some time after it is removed from the fire, that the two rods may be heated as uniformly to the same degree as possible, the pendulum is hung upon the wire, and the two indexes made to stand nearly in an horizontal position. The two graduated plates *bi* are then slid upon a wire, till the divisions in each mark'd *o* are pointed to by the indexes. As the pendulum cools, the lower index will be seen gradually to descend; but if the ends of the two screws, in the ball of the pendulum, act upon proper parts of the levers, the upper index will continue in the same place. If the ends of the screws are either too far off, or too near the centres of the levers, the index will either rise or descend; and, by comparing the number of divisions it has varied, with those which the lower index has varied, a near estimate may be made, how much the screws require to be alter'd; and, in a very few trials, they may easily be adjusted to a very great exactness. In order to make an actual trial, how far this contrivance of the pendulum will answer the end proposed, it is necessary, that the clock, to which the pendulum is fitted, be made with great exactness, and intirely to be depended on: For otherwise the experiments will be very uncertain, as I found in the clock I first made use of.

I have already observed, that, in order to render this clock as perfect as possible, I made it, in several respects, different from the common ones, in hopes of removing some imperfections I apprehended they were liable to. But as, in this attempt, I fell into an error, which it was a considerable time before I discover'd, my making the trial was thereby greatly retarded. And in order to prevent others from falling into the like mistake, I shall beg leave to give some short account of it.

In a common clock the pendulum is usually hung by a spring to a cock on the back plate of the clock, whilst the wheel and pallets, by which the pendulum is kept in motion, are placed in the middle of the frame; and the pendulum is moved by a piece of steel (call'd the crutch) riveted to one end of the arbor, to which the pallets are fastened. This disposition of the pieces I apprehended liable to some considerable objections: To remedy which, I contrived to fix the pallets to the upper part of the pendulum itself, above the centre of motion; and, in order to make the pendulum vibrate as freely as possible, it was made to turn upon two steel points, and was hung in the middle of the frame, exactly under the swing-wheel, and so as to vibrate in the same plane with it. By this means I was in hopes, that it would have moved with much greater freedom and regularity, than when hung after the common method; and, upon trial, it was found to move with so great freedom, that a pendulum of above 20 pounds weight, when hung in its place without the clockwork, and made to vibrate thro' an arch of two degrees, was found to make above

1200 vibrations, before it had lost half a degree, and was observed to have a sensible motion above 20 hours afterwards; and the clock, when first put together, was kept going, for several days, by a weight of only eleven ounces, hung to the end of a single line. But it was not long, before I discover'd, that this great freedom made it liable to be considerably affected by the least motion.

A remarkable instance of this I communicated to this Society, which was published in the *Philosophical Transactions*, N<sup>o</sup> 453. But the greatest objection to this method was, the points being subject to wear; and I found, that the least alteration in them would occasion the clock to vary much more, than (without having made the trial) I could have imagined. To remedy this inconvenience, I made the pendulum to move upon edges, like those, on which the beam of a pair of scales turns (a method I had good reason to believe had been made use of with success); but I found these likewise liable to wear, tho' not in so short a time as the points; so that, after much time spent in making several experiments, in order to remedy this inconvenience, I found myself obliged to lay this method wholly aside, and to hang the pendulum upon a spring, as usual.

In making this alteration, I observed one circumstance, which I think deserves to be taken notice of. Before I made any alteration in the work, I took particular notice to what height the pendulum required to be raised, before the pallets would escape from the wheel. I next observed the number of degrees of each vibration of the pendulum, when mov'd by the clockwork; and then, the clockwork being remov'd,

the pendulum was made to describe an arch of two degrees ; and particular notice was likewise taken, in what space of time it had lost half a degree each vibration. Having then made the necessary alterations for hanging the pendulum by a spring, and particular care being taken that the pallets should scape off from the wheel exactly at the same angle as before, the pendulum being hung by its spring, and made to vibrate thro' an arch of two degrees, it was observed to lose half a degree in about half the time it did when turning upon edges. But, upon being set a-going by the clock-work, the pendulum was found to describe an arch of near two degrees more than before : For, when it turned upon the edges, it described an arch of only three degrees ; whereas, now it was hung by the spring, it vibrated near five degrees ; which was very different from what I expected.

This alteration being made, I soon found, that the clock went very regular ; and, after a sufficient trial, was fully satisfied the pendulum would answer my expectations. But, fearing lest I might be thought prejudiced in favour of my own invention, I engaged the Rev. Mr. Professor Bliss to make trial of it ; and, accordingly, in the beginning of the year 1750, I sent to him, at Oxford, a clock for that purpose ; and, in January last, I received from him a letter, giving his opinion of it, of which the following (so far as relates to the clock) is an exact copy.

“ S I R,

“ I have now had thorough trial of the clock ;  
 “ and am perfectly satisfied, that your pendulum  
 “ takes

“ takes off the effect of heat and cold as well as  
 “ either the gridiron-pendulum (as it is com-  
 “ monly called) or the quicksilver pendulum; and  
 “ this upon sufficient trial for near two years. It has  
 “ this advantage of both the fore-mention’d ones,  
 “ that it may, by lengthening or shortening the  
 “ levers, be easily adjusted to the exact proportion  
 “ of the difference of the iron and brass, which nei-  
 “ ther of those kinds is capable of, without very  
 “ great trouble and difficulty. I was indeed preju-  
 “ diced against the method of doing it by levers, as  
 “ I had heard the late Mr. Graham say, that he had  
 “ tried levers in different ways, that he found they  
 “ did not work regularly and freely, but by jerks.  
 “ However, in your method, I am satisfied, by the  
 “ fullest experience, that they succeed as well as  
 “ either of the other sorts, or perhaps any other kind,  
 “ that may be invented hereafter.”

Before I conclude this paper, I shall beg leave  
 to acquaint this honourable Society, that, in the  
 year 1748, I made a model of a contrivance to be  
 added to a pocket-watch, founded upon the same  
 principles, and intended to answer the like pur-  
 pose, as the pendulum above described. And, at  
 a meeting of a council of this Society, on February  
 15 last, I produced a watch (which I had made for  
 a gentleman) with this contrivance added to it, and  
 likewise the model, by which was shewn to the  
 gentlemen then present what effect a small degree of  
 heat would have upon it. But, as I have not yet  
 had sufficient trial of this watch, I shall defer giving  
 a particular description of this contrivance, till I am  
 fully



fully satisfied to what degree of exactness it can be made to answer the end proposed. I am,

Gentlemen,

June 4, 1752.

Your most obedient

humble servant,

J. Ellicott.

LXXXII. *A Description of a new Tackle or Combination of Pullies, by Mr. J. Smeaton.*

Read June 11, 1752. **T**HE axis *in peritrochio*, and the compound pulley, are the only mechanic powers, which can with convenience be applied to the moving large weights, when the height, to which they are intended to be raised, is considerable. The excellence of the former is, their working with little friction; that of the latter, in their being easy to be moved from place to place, and applied *extempore*, as occasion requires.

The present methods of arranging pullies in their blocks may be reduced to two. The first consists in placing them one by the side of another upon the same pin; the other in placing them directly under one another, upon separate pins. But in each of these methods an inconvenience arises, if above 3 pullies are framed in one block. For, according to the first method, if above 3 pullies are placed by the side of one another, as the last line, by which the draught

is made (or, as it is commonly called, the fall of the tackle) must necessarily be upon the outside pulley or shieve; the difference of their friction will give it so great a tendency to pull the block awry, that as much will be lost by the rubbing of the shieves against the block, on account of its obliquity, as will be got by increasing the number of lines.

The second method is free from this objection; but, as the length of the two blocks, taken together, must be equal to the sum of the diameters of the fix pulleys, besides the spaces between for the ropes, and the necessary appendages of the framing, were there more than three pulleys in each block, they would run out into such an inconvenient length, as to deduct very considerably from the height, to which the weight might otherwise have been raised: so that, upon those accounts, no very great purchase can be made by the common tackles of pulleys alone.

In order therefore to increase its power, sometimes a second tackle is fixed upon the fall of the first; but here it is obvious, that whatever be the power of the second tackle, the height to which the weight might otherwise have been raised by the first, will be less in the same proportion as the purchase is increased by the second.

Again, very frequently the fall of the first tackle is applied to an axis *in peritrochio*, which increases the purchase very commodiously without the inconveniencies last-mention'd; but then the machine is render'd cumbersome, and, consequently, less fit for a moveable apparatus.

All those impediments I have avoided, by combining the two methods, above described, in one.

The

The pullics are here placed in each block in two tier; several being upon the same pin as in the first method, and every one having another under it, as in the second; as also that, when the tackle is in use, the two tier, that are the remotest from one another, are so much larger in diameter than those that are nearest, as to allow the lines of the former to go over the lines of the latter without rubbing.

From this construction arises a new method of new method of reeving the line upon the shieves: For here let the number of shieves be what it will, the fall of the tackle will always be upon the middle shieve, or on that next the middle, according as the number of pullics on each pin is odd or even.

To do this, the line is fixed to some convenient part of the upper block, and brought round the middle shieve of the larger tier of the under block, from thence round one of the same sort next to the centre one of the upper block; and so on till the line comes to the outside shieve, where the last line of the larger tier falls upon the first shieve of the smaller, and being reeved round those, till it comes at the opposite side, the line from the last shieve of the smaller tier again rises to the first of the larger, whence it is conducted round till it ends on the middle shieve of the upper block on the larger tier; as will appear more plain, by inspection of the figure annexed.

In this method all the lines are clear of one another, and the blocks are kept parallel. The model which I have the honour to shew the Society, and from which I made the draught, is a composition of 20 shieves, five on each pin. With this model, which may easily be carried in the pocket, I have  
raised

raised 600 weight. But with a tackle of this sort, properly executed in large, one man will easily raise a ton, and a greater number in proportion \*.

I have tried several numbers of shieves as far as 36; but 20 seems to be the largest number, that will answer well in practice.

A very commodious tackle of 12 might be executed in wood, in the same manner that common blocks are made.

I should not have troubled the Royal Society with an account of this contrivance, did it not seem promising of much utility, in a variety of purposes; particularly for merchants, seamen, builders, engineers, &c. I therefore intirely submit it to the censure of that honourable body.

J. Smeaton.

*P. S.* In constructing a tackle of 20 for 3 tons, the larger tier of shieves should not be less than 8 inches, the running line needs not be thicker than half an inch diameter, and the iron pins need not be so thick.

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\* A large tackle of 20 was tried on board one of his Majesty's ships; and by the help thereof, tho' it was with a new rope, one man raised one of the ship-guns and carriage, that together weighed 27 hundred-weight; there being a person, as usual, to hold on, or prevent the rope from slipping back.

LXXXIII. *Extract of a Letter from Wm. Dixon, Esq; F. R. S. to Mr. W. Watson, F. R. S. from Loverfall near Doncaster in Yorkshire, June 1, 1752. concerning some vegetable Balls; with Remarks on them by Mr. Wm. Watson.*

Dear Sir,

Read June 18, 1752. **I** HAVE sent you some balls, which seem to me to be plants of a very particular kind. They were taken up in a fresh-water lake, on a large common in the East Riding of Yorkshire, about twelve miles west of Hull. The lake is from one hundred to two hundred acres in bigness, according to different seasons, and empties into the Humber; which is pretty salt, and has sometimes infected it a little at very high tides. The water is very bright, and the bottom in many places is quite cover'd with these balls, like a pavement, at different depths. These now sent were about six inches under water; and many are left quite dry every summer. Whether they are particular to this place, I know not, having no description of them in my botanic library. To you, who have so general an acquaintance in that branch of natural knowlege, they may prove old acquaintance. [Thus far Mr. Dixon.]

The vegetable here mention'd, and which I take the liberty of laying before you, I have never seen till now; neither have I been able to find it described in any of the botanic writers, whom I have consulted. The matter, of which it is compos'd, is that of a *confer-va*; and should therefore have had a place under that  
genus

genus in Dillenius's *Historia Muscorum*. They are of a deep-green mossy colour, are hollow, of an irregularly spherical figure, and of different sizes, from an inch and half to three inches in diameter. They are cover'd with very short *villi* externally, and the thickness, from their external to their internal surface, is about a quarter of an inch; their texture is most compact the nearest to the surface. I should denominate them globose *conserva*.

Mr. Ray, in his history of plants, Vol. I. p. 83, describes a plant, which he found in Sicily, something like this now sent by Mr. Dixon. When treating of the *Algæ Pomum* of John Bauhin, which, according to this last, was of the colour of sponge, he says, *Quod nos in Siciliae littoribus invenimus, colore erat viridi, et propius accedebat ad bursæ marinæ Cæsalpini descriptionem; erat enim intus concavum, ex muscosa seu spongiosa scilicet capillari substantia constans, et ostiolum habebat rotundum, qua lapidibus adhærebat.*

The plant now before you wants the *ostiolum*, by which it adheres to the rocks, taken notice of by Mr. Ray, in all the specimens I have seen; and, from its mossy substance, can by no means be ranged under the genus of *alcyonium*, where Mr. Ray has given us the passage just now mention'd.

LXXXIV. *A Letter from the Rev. William Henry, D. D. to the Right Honourable the Lord Cadogan, F. R. S. concerning the Copper-Springs in the County of Wicklow in Ireland* \*.

My Lord, Ann-street, Dublin, Apr. 18, 1752.

Read June 18, 1752. **H**AVING, in my progress to visit the charter-schools, passed by the rich copper-mines in the county of Wicklow, I judge, that it will not be unacceptable to your lordship to receive some account of them.

These mines lie in the southern part of the county of Wicklow, upon the river Arklow, on each side of that river, and about seven miles westward from the town of that name, among hills, that rise to the height of small mountains.

The mine, which was formerly wrought on, is that of Ballymurtogh, on the south bank of the river. It yielded vast profit to the undertakers ; but, on account of some difference between Mr. Whalley and the company, it has been disused for some years past.

This is amply compensated by the far richer mines of Crone-Bawn (in Latin *Corona alba*) on the north side of this river.

Crone-

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\* An account of springs of the same kind in Hungary may be seen in Dr. Edward Brown's *Travels*, p. 68, 69, edit. 1685, fol. Count Marfigli's *Danubius Pannonico-Mysicus*, tom. III. p. 25. and Matthius Belius's *Notitia Hungaria*, tom. II. p. 393, 394. There is published, in the *Giornale de Letterati d' Italia*, tom. XXVII. Art. IV. p. 186, & seq. a second letter of Signor Agostino Soderini, of Venice, relating to the art of metallurgy, in which he describes the method of changing iron into copper by vitriol.

Crone-Bawn is an hill of two miles in circumference, and, as near as I can guess, about 1000 feet in height, swelling regularly in the form of a large inverted bowl. The bowels of this hill are, on all sides, full of rich mines, as appears by the shafts, which have been sunk in different parts of it. But the principal works lie on the east side, about half way up the hill. Here I saw several shafts, sunk from 50 to 70 fathoms deep, as the directors of the works informed me. In sinking these shafts, the first mineral met with is an iron stone. Beneath this, they arrive at a lead ore, which seems mix'd with clay, yet yields a large quantity of lead, and some silver. Under this lies a rich rocky silver ore, which sparkles brightly, and yields seventy-five ounces of pure silver out of a ton of ore, beside a great quantity of fine lead.

Having pierced some fathoms thro' this, they arrive at the copper ore; which is very rich, and may be pursued to a vast depth.

There are five hundred men employed in these mines; and having inquired from several of them, how they could live in these caverns? they said, that they had their health very well; and that there was a particular quality in the copper-water to cure, immediately, all sores in their skin or flesh. Their pay is eight pence a day.

In order to carry off the water from the mines, there are levels carried on a great way under-ground, from the lower part of the hill. Out of these levels issue largest reams of water, most strongly impregnated with copper.

An accidental discovery, which happen'd not long ago, is like to make these streams more beneficial  
than



than all the rest of the mines. Some of the workmen, having left an iron shovel in the stream, found it some weeks after incrusted with copper, insomuch that they thought it converted into copper. This gave the hint of laying bars of iron in these streams, which is done in the following manner :

Oblong pits are dug, ten feet long, four wide, and eight deep : the bottom laid with smooth flags ; the sides built up with stone and lime, with wooden rude beams across the pits to lay the iron bars on. Chains of these pits are continued along the stream, as far as the directors please ; for the water never abates of its quality, if it were convey'd from pit to pit thro' a thousand. Soon after the iron bars are laid in these pits, they contract a copper rust, which, by degrees, intirely eats away the iron. The copper, which is in the water, being thus continually attracted and fixed by the iron, subsides to the bottom of the pit. To hasten this dissolution, the iron bars are sometimes taken up, and the rust rubb'd off them into the pit. In the space of twelve months the whole bar is commonly dissolved, if the iron be soft ; for steel or hard iron will not do here. The stream is then turn'd off the pits ; and the men with shovels throw up the copper, which lies on the flag at the bottom, like reddish mud. This mud, being laid in an heap, and as soon as dry, becomes a reddish dust ; of which I send your lordship an ounce, that I took up on the spot. 'It is then smelted into copper.

This being the apparatus, the product is thus. One ton of iron in bars produces a ton and 19 hundred and an half weight of this copper mud or dust. Each ton of this mud produces, when smelted, 16 hundred

hundred weight of the purest copper, which sells at ten pounds *per* ton more than the copper, which is made of the ore. There are about 500 tons of iron now laid in these pits; and the proprietors may, with proportionable advantage, lay in many thousands.

The water, that runs from these mines, enters the river Arklow on New Bridge; and is of so corrosive a nature, that no fish can live in this large river from hence to the sea.

If your lordship thinks these accounts worth notice, you may communicate them to such of your friends, and other curious persons, as you please.

I am, with the sincerest respect,

Your lordship's most obedient,

and most humble servant,

William Henry.

**LXXXV.** *Extract of a Letter to Dr. Maty, F. R. S. from Geneva, concerning the Introduction and Success of Inoculation in that City.*

Read June 18, 1752. **I**N September 1750, the practice of inoculating the small-pox was first introduced into Geneva. The example was set by a young lady; and was, the next year, follow'd in the hospital of foundlings, where it was admitted by an order of the governors, and authorized by the magistrates.

trates. Their method of doing it was generally the same, which is now commonly used in England; whence instructions were sent to Geneva, when they first began to inoculate. Yet three persons were inoculated in a new manner. These were blister'd slightly, by means of a small vesicatory applied to that part of the arm, where the incision is usually made. The blister occasion'd by this plaister was open'd, and a pledget dipp'd in the pocky matter was applied to the excoriated part. In one instance the incision was made only in one arm; the success of which was the same, as when it had been made in both. Some pocky matter was made use of, which had been kept three weeks; and some, that had even been kept four months, without any apparent difference in the effects from that which was fresh; unless it was owing to this, that, in one instance, the small-pox came out four days later than the usual time.

The experience, which they have hitherto had in Geneva, has suggested to them a conjecture, that the incision ought to be made deeper, where the matter, which is used, has been kept some time. All, who have yet been inoculated in Geneva, have recover'd; and the far greater number of them have had but an inconsiderable number of pustules.

LXXXVI. *A Letter from James Parsons,  
M. D. F. R. S. to the Rev. Mr. Birch,  
Secr. R. S. concerning the Formation of  
Corals, Corallines, &c.*

S I R,

Read June 18, 1752. **T**HE several ingenious opinions of some of this learned Society, upon what M. Peyssonnel has advanced, concerning the formation of some of the submarine bodies by animals, have occasion'd the following conjectures, which I lay before you, not at all presuming absolutely to decide a question of so difficult a nature, but only to endeavour at throwing a little more light upon the subject, in general, by such further observations, as I thought would be most conducive at least, to come to a little more certainty about it.

I believe it may be said, that there can be no ocular demonstration of the fabrication of any of these bodies, whether by animals, or by vegetation; because this happens under the water, far enough from any human observation. Therefore, when at any time such of these, as are said to be the work of animals, have been taken up, there is no doubt, but that those soft gelatinous weakly animals may have been seen upon them, and thence have been concluded to be the makers of them. Certainly there is nothing impossible to Divine Providence, in the order and disposition of every thing to the best advantage. Among the animals, from the largest to the most minute, none are destitute of proper habitations; and we see, amongst  
S f f them

them, prodigious variety in the modes and designs of such dwelling-places. Some are capable of erecting for themselves commodious apartments to live in, as shell-fish, even out of their own constituent parts as they grow. Others lodge their young in the very skins of animals ; and where there are any, who have neither sagacity nor strength enough to provide places for themselves, they are at least taught by their Maker to find them ready made.

Such are the bounds set to our intellectual powers here, that we can have no means of judging of objects, which do not immediately fall under our inspection, but by comparing them to something else, as near them as may be ; or by considering their proportions and effects ; what is probable, what is not, in the phenomena, that belong to them ; and what absurdities may arise from the uses and actions ascribed to them ; for certainly they may be easily seen, by considering the objects themselves.

I would neither conclude, with M. Peyssonnel, that, because I found animals upon such bodies as he mentions, they were the makers of such bodies ; nor that, if one or more kinds of those bodies were actually the work of such creatures, all others, that had any relation to them, must also be their work ; any more than I would, on the other hand, conclude, that, because one or more of these submarine substances were not made by them, none at all were produced by them. I would rather examine the parts of those bodies in as nice and scrupulous a manner as possible, and compare their characteristics with those of other bodies in both the animal and vegetable kingdoms ; and, by finding out some of their

their properties only, be, in a great measure, able to range them in the rank, which they were designed to hold by Divine Providence.

In order to this, let us see first what are those animals, which we are acquainted with, who certainly fabricate their dwelling-places, as they grow for themselves; and what the common or usual advantages are, which they are in general observ'd to be endow'd with; which will be best done, by taking a near view of them. All the testaceous tribe, whether of land or water, and whatever their forms be, may be said to produce their own habitations, but not to fabricate them. For we must observe, there are but two modes, by which these kinds of animals are furnished with them; the one by secretion from themselves, and these necessarily grow with them; the other by a design'd apposition of parts of the animals themselves. Now, in the first case, there is a necessity for a just proportion between the animal itself, and the shelly matter secreted from it. It must be large enough, and have stability and strength in proportion to the matter which it secretes, and is to move about with; and it will appear, that this is the general rule thro' nature: Or, if it be an immoveable body, the creature ought certainly to be allow'd so much significancy and strength, as would, on the one hand, seem necessary for the secretion of so much matter, as was sufficient to constitute that body; or, on the other, to be capable, by its own proper action, of gathering together the matter, and building up the structure. Where this is wanting, I, for my own part, would be far from hastily concluding such work to be the fabrication of such

feemingly weakly insignificant animals; more especially if, upon these very bodies, there were appearances of other characteristics, that, at the same time, were likely to lead me into another arrangement of them.

I have produc'd here before you such of the shell-animals, as are unquestionably the makers of their own houses, that are furnish'd with these requisites mention'd; and others I shall now offer you, being somewhat nearer those said to form the coral, &c. than other testaceous kinds.

The *dentalia* are tubular shells, formed from their inhabitant animal, as much as a cockle, or an oyster: and we must observe, that each of these has a sufficient cavity for its habitation, and in itself has the proportional size and strength necessary for the purpose.

The *vermiculi marini* enjoy the same privileges, and are always attach'd to their shells at their posterior extremities, as well as the others, of whatsoever kind. They are found in groups, adhering together by a natural cement, blended, and, as it were, confounded together; and yet every one has its own cell, and is sufficient to produce in those requisites before-mention'd. All the kinds of these have one extremity small, and increase in diameter to the anterior extremity; which is indeed the case of all the turbinated fish of whatsoever kind. To these we may add, that the crusts of crustaceous animals, and those of insects in their chrysalis state, will always shew, how necessarily an animal must have power and sufficiency to form his habitation, either by secretion, or actual operation.

The *syringoides*, so call'd from their forms, carry the same testimonies of their strength and power;  
many

many species of which we find fossil, of which I have the honour to shew several specimens: And I have no doubt, but it will be hard to find any creatures more deficient, or, in other words, more abandon'd to destruction by the Creator, than these, in any part of nature.

Whatever is constructed by an animal, that is, among those, that we know with any certainty, it is surely to dwell in themselves, or to deposit eggs or young in. There was really no need to build a fabric to dwell upon; because all those creatures, such as the polypi of every kind, which attach themselves to bodies, have innumerable sorts of matter, to which they can adhere every-where, near them: And if these of the sea have, in their nature and properties, any analogy with our fresh-water polypi, as to their propagation, and the detachment of their young from themselves; with the several kinds of the same genus, the *polypes à panache*, *polypes à bouquet*, the bell-like polypi, and every other kind, discover'd by our ingenious observer Mr. Trembley, all which detach their young from them nearly in the same manner; one would almost be persuaded, that they were never intended to dwell in cavities, but upon *nidus's* convenient for their attachment only, with full liberty, at proper times, to detach their young in like manner; who immediately meet some or other of these submarine bodies for their security also; for indeed there is hardly room to suppose any other way of propagation for these, than for those of M. Trembley, since they are much of the same substance and consistency every way. And it must be remark'd, that few or no animals, that have  
shells



shells of any kind, can ever quit them, but must remain in them till they die.

We are now, secondly, to consider some of the most obvious marks, that distinguish vegetable from other substances.

Whatever body is fixed by its root, no matter, whether it be flat or fibrose, increasing upwards, and ramifying into smaller and smaller branches, till they become more and more pointed to their extremities; having fibres either apparently tubular, or only porous or woody, would incline one, who had at all made the works of nature his study, rather to favour the idea of a vegetable in such a body, than that of any other production. If these characteristics are common to any of the species of corals, corallines, madrepores, &c. it would be no wonder they ow'd their increase to a kind of vegetation; nor would their hardness weigh at all against it, because every one knows, that water is the universal vehicle of all matter into bodies of this kind. It is by water, that the testaceous matter is carried into the juices of shell'd fish, and from it detach'd into the order we see it in the shells. It is from water, that sparry incrustations upon vegetables are made: It is a deposit from water, that lines our common tea-kettles with a sparry crust: And it is also this fluid, that conveys the particles of tartar into the grape, which is afterwards deposited upon the sides of the wine-vessel; and no doubt but it is water, which carries up into those hard bodies their stony matter; for there can be no doubt of their being organized bodies. Besides, tho' the organization, in its origin, is probably flexible enough, yet the arrangement of these

these petrific particles in so exact a manner would inevitably render the whole hard enough, in the course of its growth. Is not the shell of a common egg hard enough? and yet its membrane, into the cells of which the testaceous particles were secreted and ranged, in order to produce that hardness, was soft enough before.

If we were to make transverse sections of the generality of these bodies, we should see a regular radiated order of pores from their central medullary pipes, some foliated, others more tubular, others barely porous, all differing from one another only according to their own natures. What more is there in the order of the fibres of trees or plants? Transverse sections of any of these will shew you the most beautiful figures, in such orders, that can be conceiv'd; which, long ago, that accurate and learned naturalist Dr. Grew has ingeniously observ'd, in his *Anatomy of Plants*, where he has given elegant figures of such sections in a variety of examples. And altho' some of these bodies have their pipes and pores quite stopp'd up, as they grow, yet their external appearance will shew them fibrose.

In like manner some trees are so very hard, from the strong connection of their parts, that, in a transverse section, neither pores nor fibres can be distinguish'd; and they are as susceptible of a fine polish as any stone. And indeed it would seem to me much more difficult to conceive, that so fine an arrangement of parts, such masses as these bodies consist of, and such regular ramifications in some, and such well-contriv'd <sup>organs</sup> ~~organs~~ to serve for vegetation in others, should be the operations of little, poor, helpless, jelly-

jelly-like animals, rather than the work of more sure vegetation, which carries on the growth of the tallest and largest trees with the same natural ease and influence, as the minutest plant, in a manner, which I have elsewhere explain'd.

Is it not also somewhat particular, that, if corals are the work of these insects, there should be no cavity left behind them, as they raise it into branches; but that they should leave it solid within? And would it not be very surprising, that such cellular passages, as we see diversified into many kinds, should be made by these creatures from the basis, to be left behind them, as they carry up the building, without any further purpose, in brain-stones, &c? If this was the case, and that these little creatures could be supposed to build them, there would be a deviation from the general uniformity and purpose, that is observ'd every-where else: For certainly cells are built by every animal to deposit something, eggs, young, or other matter, in them; neither of which can be said of the insects in question.

It has been said, that flies, wasps, and bees, build themselves cells; in order to make a comparison between them and these polypi. They do so; but is there no distinction to be made? I can find several. Bees, wasps, &c. are in themselves, compact strong animals, well made for the work allotted them, very able to bring and put together the materials of their nests; and when they have done their work, that proportion between the fabric, and the creatures which raised it, is apparent, which all nature points out, and the purpose is fulfilled soon, in their building them with what nature had destin'd they should

should hold. But can this be said of our polypi? Where is that proportion between a little configured jelly, and the mass of matter said to be their work? What is deposited in the cells they form? What makes others solid? And how do these jellies so wonderfully dispose the fine arrangement of pores, fibres, nodes, branches, &c.? And to what purpose, if they could be supposed capable of it? In a word, I humbly purpose to sum up this essay in two general sentiments; and these will be the rule, by which I, for my own part, shall always judge of things of this nature; *viz.*

1. Whatever bodies shall be found to carry the appearances and characteristic marks of vegetables; even tho' animals are found upon them, they certainly will pass with me for such, till stronger evidence shall evince the contrary. And,

2. I shall ever expect to see, at least, a seeming power, proportion, and stability, in animals, to render them capable of performing what they may be thought to have done. I am,

S I R,

With the utmost respect,

Yours and the Society's

most obedient servant,

J. Parsons.

LXXXV VII. *A further Account of the late Plague at Constantinople, in a Letter of Dr. Mackenzie from thence, of the 23 of April 1752, to John Clephane, M. D. F. R. S.*

Read June 18, 1752. **A**S a corollary to my former account

sent to Dr. Mead, please to know, that, on January 3, 1752, there was an accident of the plague, when the thermometer was at 53. Jan. 24, another accident, therm. 52. Jan. 26, an accident at Buiukdere, therm. 51. Feb. 8, accidents at Cassim, Pacha, and Phanar, therm. 52. Feb. 10, an accident in Galata, therm. 55; patient recover'd. Feb. 15, another accident in the same house, therm. 53. March 8, an accident in Galata, therm. 56; and not one accident since, tho' at present the thermometer is at 50, and has been at 44 the 16 instant; so that we have great hopes to get clear, if no infection is convey'd to us from any other quarter.

To satisfy you, how I came to be so exact in dates, it is proper to inform you, that I have kept, ever since I have been in Turkey, a journal of the thermometer, barometer, winds, weather, diseases, and other events; which I mark down exactly twice every 24 hours.

Prosper Alpinus observes, that the Etesian winds at Cairo remove the plague intirely; so that they fear nothing after these winds begin. And I can assure, *bona fide*, that all the plagues, which have been at Smyrna and Constantinople for the last twenty  
I years,

years, have been hottest and most violent during the season of the Etesian winds; still allowing, that, were it not for the Etesian winds, the plague would be more violent in the hot months. Witness the 24 of June 1735, there being no wind, the sickness ravaged more than any other day, while it lasted.

I return you many thanks for shewing my remarks to the Royal Society. I am sensible they have no other merit imaginable, besides their being true, which may be a motive for some of the best taste to relish them.

As I hear there is a bill to be brought into parliament to regulate quarantines, I will give my humble opinion of them, as they ought to be observed in Great-Britain and Ireland.

1. It seems to me useless to put a ship's company from the Levant in quarantine in Britain. For how is it possible, that men, who have been one or two months at sea, toss'd about with different winds and weathers, and arriving, after such a time, in good health in England, can have any infection in their bare bodics? Wherefore, as soon as they arrive, they should be stripp'd naked, and have clean linen and cloaths put on, and then sent immediately ashore. This would save to the owners of ships these sailors wages and victuals during the quarantine; and the sailors might go to sea again, without eating the bread of idleness for so many days. The case is different in Italy, and in the south of France; to which countries a ship with a fair wind may perform a voyage in eight days from the Levant; during which time a person may have the plague about him,

him, without being confin'd to his bed; of which there are many instances.

2. There should be found very honest men to be overseers of the lazaretto, who will take the trouble of seeing all the goods unbaled, and every particular parcel expos'd to the air: Otherwise, if the goods remain in the same place, and every shred not expos'd to the air, they may be as well in the merchants magazines as in the lazaretto.

3. No person, during quarantine, should be allow'd to go near the lazaretto, excepting such as have the care of it; for fear strangers, going too near, may receive infection from the goods in the lazaretto, and, at their return, communicate it to others.

4. There should be one or more doctors to attend the lazaretto, and take care of the people, who look after it, in case any of them should be sick, that their sickness may immediately be known: And, should it prove contagious, the patient should be separated, and so the infection, as much as possible, hindred from spreading.

5. The ships should be very well clean'd and perfum'd in the hold, and between decks, where the goods lay during the voyage, for some days after they are unloaden. Otherwise persons going into the hold of the said ships, may be infected, and communicate the infection to others. This I communicate to you, as you have an opportunity of conversing with several members of parliament.

LXXXVIII. *A Letter of Mr. James Short,  
F. R. S. to the Royal Society, concerning  
the Inventor of the Contrivance in the Pen-  
dulum of a Clock, to prevent the Irregula-  
rities of its Motion by Heat and Cold.*

Gentlemen,

Read Nov. 9, 1752. **T**HE subject of conversation of late having often turn'd upon that ingenious contrivance in the pendulum of a clock, to prevent the inequalities in its motion, arising from its different lengths, in different seasons of the year, by the effects of heat and cold; and it having been often asked, who was the inventor of it, I have therefore thought proper to draw up the following historical account of it: And as this account contains nothing but matters of fact, supported by the best authorities, I hope it will be acceptable to this Society. I am

Your most obedient humble servant,

J. Short.

**S**OON after the invention of pendulum-clocks (justly ascribed to the celebrated Mr. Huygens), it was found, that they were liable to considerable inequalities in their motion; which were imagined to arise from the pendulum, in its vibrations, describing an arc of a circle; and, consequently, that the larger vibrations must be slower than the shorter ones.



ones. In order to remedy this imperfection, the same Mr. Huygens wrote a treatise, called *Horologium oscillatorium* (a piece of geometry, which does honour to the last century), in which he demonstrates, from the properties of the cycloid, that the vibrations of a pendulum, moving in a cycloid, would be perform'd in equal times, even tho' the vibrations were unequal. Pendulums therefore were made to vibrate in a cycloid; but great inequalities were still observ'd in the motion of clocks.

We do not read of any attempts, after this, to regulate the motion of clocks, till the year 1726, when Mr. George Graham deliver'd into the Royal Society a paper, which is publish'd in the *Phil. Trans.* N<sup>o</sup> 392, in which he says, that it having been apprehended, that the inequalities in the motion of clocks arose from a change of length in the pendulum, by the influences of heat and cold, he, about the year 1715, made several trials, in order to discover, whether there was any considerable difference of expansion between brass, steel, iron, silver, &c. when exposed to the same degrees of heat; conceiving, that it would not be very difficult, by making use of two sorts of metals differing considerably in their degrees of expansion and contraction, to remedy, in great measure, the irregularities, to which common pendulums are subject. He says also, that, from the experiments he then made, he found their differences so small, as gave him no hopes of succeeding that way, which made him leave off prosecuting this affair any more at that time: That, some time after, having observed an extraordinary degree of expansion, by heat, in quicksilver, he thought of a proper manner

manner of applying a column of it to the pendulum of a clock, in order to prevent the inequalities arising from its different lengths by the effects of heat and cold; which succeeded accordingly, and is what is now called Mr. Graham's quicksilver-pendulum.

Mr. Graham, in the same paper, takes notice, that, tho' the pendulum of a clock was to remain invariable, yet there would still be some irregularities in the motion of the clock, arising from the friction of the different parts of the clockwork, and from the different degrees of foulness.

In the year 1725, Mr. John Harrison, of Barrow in Lincolnshire, made several experiments upon wires of different metals, in order to find their different degrees of expansion and contraction: For he thought, that, by a proper combination of wires of two different metals, differing considerably in their expansion and contraction, he might be enabled to keep the centre of oscillation of a pendulum always at the same distance from the point of suspension. In consequence of these experiments, he made a pendulum, consisting of one steel wire, at the end of which is the bob or weight, and, on each side of this wire, four wires alternately brass and steel, so disposed and contrived, as to raise the pendulum the same quantity as it is lengthen'd by heat, and to let down the pendulum in the same proportion as it is raised by cold. He made also a drawing of a clock, in which the wheels are disposed in a different manner from those then in use; which drawing I have seen, signed by himself in the year 1725. Two of these clocks with pendulums, as described above, were finished in the year 1726. In these clocks Mr. Harrison has made a particular

a particular sort of pallets, so as to be almost intirely free from friction; for tho' he had thus happily succeeded in his contrivance to prevent the inequalities in the motion of the clock, arising from the different lengths of the pendulum by the effects of heat and cold, yet he found there were considerable errors still remaining, occasion'd by the friction of the pallets, as in the common way. He has also suspended the pendulum upon the wall of the house, intirely independent of the clock and clock-case: For he had observed considerable alterations in the going of the clock, when the pendulum is suspended as in the common manner. His pendulum vibrates in an arc of about 15 degrees, with a bob of about three pounds, between cycloidal checks, which he himself found were necessary, tho' he had never heard of M. Huygens's book, till after he had made them. He has also disposed the force of his pendulum-wheel upon the pendulum, by his sort of pallets, in such a manner, that the vibrations of the pendulum will not be affected by the different resistance of the air. Upon the whole, this clock is made in such a manner, as to be almost intirely free from friction; in consequence of which he uses no oil, and therefore there is no necessity ever to clean the clock. When he settled in London, he sent for one of these clocks from the country, and set it up in his house in Orange-street, in the year 1739, where it has stood ever since, and in all that time has never varied above one minute from the truth. He can depend upon it to a second in a month.

About the year 1729, Mr. Harrison made his first machine for measuring time at sea, in which he has  
likewise

likewise applied this combination of wires of brass and steel, to prevent any alterations by heat and cold. In the year 1726, he went on board one of His Majesty's ships of war with this machine to Lisbon, and returned, where this machine was seen by every curious and ingenious person, who were pleased to go to his house. Since that time, he has made two more of these machines or clocks for keeping time at sea, in both which he has likewise this provision, to prevent the effects of heat and cold.

An account of these curious machines, and of the many ingenious contrivances which Mr. Harrison has made use of in them, for answering their intended purpose, and also an account of the success of his voyage to Lisbon, and back again, is contained in an excellent speech of our worthy President Martin Folkes, Esq; upon his delivering to Mr. Harrison the gold medal of Sir Godfrey Copley; which speech is inserted in the minutes of the Society in the year 1749.

Mr. John Shelton, who was the principal person employed by Mr. Graham in the making of astronomical clocks, informs me, that Mr. Graham, in the year 1737, made a pendulum consisting of three bars, *viz.* one of steel, between two of brass, and that the steel bar acted upon a lever, so as to raise the pendulum, when lengthened by heat, and to let it down, when shortened by cold. This lever, which is very strong, rests upon a roller; which roller is made moveable, so as to adjust the arms of the lever to their true proportion. The whole was made to be as free from friction, as possible, in such a construction. Mr. Graham made observations, by transits of  
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the fixed stars, of the motion of the clock with this sort of pendulum, and from the experience of several years (during which the clock was kept constantly going) he found, that the clock was liable to sudden starts and jerks in its motion. Of this he informed Dr. Bradley, Mr. Blis, myself, and several other gentlemen. This clock still remains in Mr. Graham's house, in the possession of his executors.

I have been informed, that one Mr. Frotheringham, a quaker, of Lincolnshire, caused a pendulum to be made, consisting of two bars, one of brass, and the other of steel, fasten'd together by screws, with levers to raise or let down the bob; and that these levers were placed above the bob. This clock I have seen, and was told by the maker, Mr. John Berridge, that the pendulum of it was made in the year 1738, or 1739, and that the dial-plate of it was engraved at Mr. Sisson's house in the year 1738: and this clock is in the possession of Mrs. Gibson, in Newgate-street, who has had it ever since the year 1739.

In the *Hist. of the Royal Acad. of Sciences at Paris*, for the year 1741, there is a memoire of M. Cassini, in which he describes several sorts of pendulums for clocks, compounded of bars of brass and steel, and applies a lever to raise or let down the bob of the pendulum, by the expansion or contraction of the bar of brass. He has also given us, in the same memoire, a problem for finding the proportion, which the two arms of the lever should have, to answer the intended purpose; and also a demonstration of it.

In June, 1752, Mr. John Ellicott gave in to the Royal Society a paper, containing the description of a pendulum, consisting of two bars, one of brass, and the other of iron, fastened together by screws, with two levers in the bob of the pendulum, so contrived, as to raise and let down the bob, by the expansion and contraction of the brass bar; and also to adjust the arms of the levers to their true proportion \*. He says, that he first thought of these methods of applying bars of brass and iron to prevent the irregularities of a clock, arising from the different lengths of the pendulum, by the effects of heat and cold, in the year 1732; and that he put this his thought in execution in the year 1738.

In the year 1743, I bought a clock of Mr. Graham, which he had kept going for two years before. This clock has a pendulum, compounded of wires of brass and steel, in the manner of Mr. Harrison's combination. It has also a provision in the bob, to adjust the wires, in case they happen to be too long. When I first took notice of this contrivance or provision in the bob, I asked Mr. Graham the reason of it; who told me, that, having observed some inequalities in the motion of the clock, he imagined, that they arose from the wires being somewhat too long; and there-

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fore

\* He has also given us in the same paper another construction of a pendulum to prevent the effects of heat and cold, consisting of two bars, one of brass, and the other of iron; the brass bar acting upon a lever, at the end of which is fastened the pendulum, the whole so constructed and contrived, as to raise the pendulum, when it is lengthened by heat, and to let it down, when shortened by cold.

fore added this contrivance, to adjust the length of the wires ; but that, when he had done this, he found inequalities still remaining; and therefore justly concluded, that they arose from the difference in the friction of the different parts of the clockwork, occasioned by the differences in the fluidity of the oil, &c.

From what has been said above, it appears, that the improvement of clocks, by a contrivance to prevent their inequalities arising from the different lengths of the pendulum, in different seasons of the year, by the effects of heat and cold, was first thought of, and executed, by Mr. George Graham ; and that the application of wires or bars of two metals, which have different degrees of expansion or contraction, to prevent the same inequalities, was also first thought of by Mr. Graham, and first executed by Mr. John Harrison, without the least knowledge of what Mr. Graham had done before him.

LXXXIX. *A Letter from Mr. Henry Eeles, to the Royal Society, concerning the Cause of Thunder.*

Gentlemen,

Lismore, Ireland, June 18,  
1752.

Read Nov. 7, 1752. **T**HE greatest men of most ages having thought it worth the while to inquire, what was the cause of thunder ; and the world seeming to acquiesce in an hypothesis subscrib'd by some great modern names, it must appear presumptuous in me, to offer you some thoughts for a theory intirely new (at least it is so to me) unless I can shew, that the former hypotheses are ill-grounded,

grounded, and far from being satisfactory. In order to which I shall only object to the latest, (to avoid prolixity) which now has the general consent.

I think the basis, that this hypothesis stands on, is the authors assuming an analogy between thunder and fired gunpowder; and then proving, that there are sulphureous and nitrous particles in the air, they leave them to take fire by fermentation, or some other accident, and from thence to form thunder.

First, the analogy is not just; for there is not any thing similar to thunder in fired gunpowder, except the noise; which may be shewn from the different direction of their fire, and their very different effects. Fired gunpowder acts from a centre to a circumference, with equal force at equal distances every way, by propelling the circumambient air by the explosion it makes. The fire of thunder acts in rectilinear angles, (as I have often seen, and as any body may, who will observe it) with such subtil and distinct effects, as cannot be explain'd or imitated by the fire of gunpowder; the history of which effects is too well known to need a repetition here

I shall go on to shew some insuperable difficulties in the formation and firing of this supposed aerial gunpowder. And first, I think it inconceivable, that the sulphureous and nitrous particles should coalesce with some other unknown third body, in the place of charcoal, in such exact proportion, as is necessary to make gunpowder of any perfection, and to form a body compact enough to equal the noise of thunder, when fired in the open air. For such a body must necessarily descend by its own gravity, long before it arrives to a bulk sufficient for the purpose. And, secondly, I think it contradictory to all experience, that such a  
collision



collision of nitrous particles should ever happen in the common seat of thunder, which is in the most collected showers that descend: For there the nitrous particles must be absorb'd and dissipated in the water; in which state I think it impossible for them to take fire.

These, and many other considerations, too prolix for the compass of a letter, induced me to search for some other cause of thunder; which I think I have discover'd in that fire, which is made apparent in electrical experiments. This fire pervades and adheres to most bodies; while it flies, and cannot be brought to mix with some particular bodies. I shall here only mention two; air, which it flies and shuns, and water, which it more intimately pervades than almost any other body. I must also observe, that this fire does not only pervade bodies, but that it surrounds and covers them to a certain distance from their superficies, in proportion to the state of its activity, which is increased by heat: And that, when it is artificially or accidentally protruded upon any body beyond its natural affection, it will fly off to the next approaching body, which is not so much impregnated with this fire; and, when it departs in any considerable quantity, it makes a great noise or crack: All which is demonstrated by electrical experiments. Now, to shew, that this fire is the real cause of thunder, we need only consider it attending every vessel of humid vapour rising into the atmosphere, and covering its superficies to a certain depth; which I think it must certainly do. I shall not here speak my opinion how far this fire is the cause of vapours ascending, because I shall trouble you with  
that

that hereafter. Having got the vapour aloft attended by this fire, without assigning any cause for its ascent, so, without assigning any cause for its descent, I shall let it come down as usual, which is in drops much larger than the vesicles, in which it ascended. Now, in the collision to form these drops, we must consider what becomes of our fire; for the surface of these larger drops increasing only as the squares, but their solids as the cubes of their diameters, the fire, which surrounded the superficies of the vesicles, must be protruded to a much greater distance from the superficies of the larger drops, and by that means made more in proportion to the larger drops, than its natural affection would have made it join them with; and, consequently, render'd more apt to fly off to the next approaching or approached body, not so fully impregnated by this fire.

I have observed before, that the constant seat of thunder is in those clouds, which are most compact of humid vapour, and which descend in the heaviest showers, and that generally in warm weather, when the adjacent atmosphere is serene; so that the humid vapours are almost all collected into this chain of clouds; where, according to the compaction, there will be a body of this fire collected, and ready to fly off, sufficient to perform the greatest effects of thunder. Which may be easily computed, from the force of electrical experiments, where the smallest portion of this fire, flying off from an electrified body, makes an audible crack, and is able to give a considerable shock. What then must be the force of this fire, when it is so collected, as to break from a cloud in a body of fire two or three hundred yards in length?

length? which I have often seen. Now some of these clouds coalescing in their descent, and the drops increasing in their magnitude, there is a vast body of this fire collected more than what would naturally adhere to those drops and their surfaces; which being render'd more active in its vibrations, by the heat of the lower part of the atmosphere, the sphere of its affections (pardon the word, for I have no other) is also increas'd in proportion to the body of fire, which enables it to fly off to clouds, not so much impregnated, at a considerable distance, with that violent crack so much taken notice of; tho' it is far from being the most wonderful of its effects; the dire influence of which we often happily escape, by this body's being dissipated by the heat of the lower atmosphere, before it comes within the sphere of its affection for bodies on the surface of the earth. There is a subsequent rumbling noise heard after the first crack or cracks of thunder, (for this fire does not all break off from one point) which has been taken notice of, and oddly accounted for; but I think it neither is nor can be more than echo's from adjacent clouds, which at this time are generally dense enough for that purpose; and the noise growing fainter in proportion to the times of its being return'd, I think sufficiently proves it.

As to the subtil effects of thunder, I shall leave you to compare them with those of electricity, only allowing for the different force of fire, which is so much greater in thunder than can possibly be procured from artificial experiments; and I believe, that the analogy will plainly appear. I shall only hint, that, where one body has been injured by thunder;  
and

and another, tho' in contact with it, has remain'd untouched, the latter will be found to be of that kind, which electrical fire will not join with.

I must beg you will let me know, whether this theory is worth your acceptance; for I fear I am, like a fond mother, blind to the imperfections of my own child. I have dandled this opinion for eighteen months past; still fearing to lay it before you; and now, instead of seeing its defects, I begin to fancy, that it has the face of truth and demonstration. If you think this discovery worth the pursuit, I shall venture to trouble you hereafter with some farther attempts to shew, that this fire is a most considerable agent in nature. First, that the ascent of vapour and exhalation is principally owing to it, and that our atmosphere, by that means, is kept more homogeneous than is generally supposed, and fitter for respiration, vision, &c. and that clouds of heterogeneous matter are kept suspended at their usual height merely by this fire. Secondly, I shall prove, that this fire is the cause of reflexion, refraction, and inflexion of light. Thirdly, I shall endeavour to shew, that it is the cause of that secondary attraction and repulsion, which Sir Isaac Newton has taken notice of. Lastly, I shall give some hints of the great use of this fire in animal life, and in vegetation. What further I have thought of this fire, I shall not now trouble you with. I am,

Gentlemen,

Your most humble and  
most obedient servant,

Henry Eccles.

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XC. *Extraëts of two Letters of Thomas Hope, M. D. to John Clephane, M. D. F. R. S. concerning Monsieur Daviel's Method of couching a Cataract.*

Read Nov. 16, 1752. **S**INCE I received your last, I had heard of a new method of performing the operation for the cure of the cataract, but did not care to say any thing of it, until I had seen it myself, and had inquired into the success of it. M. Daviel, a surgeon of this place, was the first, who, in 1745, began to put it in practice, and has at last brought it to perfection; of which he has given a memoir to the Academy of Sciences of 115 operations, 100 of which have succeeded. A few days ago I saw him perform it on two persons, of which take the following description :

After having placed the patient in a right light in a chair, he places himself over-against, and somewhat higher than, the patient : an assistant holds the head steady, another keeps the upper eye-lid open ; he, with his left hand, keeps open the lower eye-lid. Then he takes an instrument like a lancet, of a myrtle-form point, a little crooked upwards, and fixed in a handle, and, making the patient look upwards, he pierces the *cornea transparens* at its lower circumference, just where it joins the *sclerotica*, conveys the point of the instrument between the *cornea* and *iris* upwards, beyond the pupil ; he enlarges this opening on each side by the same instrument : he then takes  
out

out this instrument, and introduces another of the shape of a narrow lancet, made round at the point, fixed in a handle: with the cutting sides of this he enlarges the opening. Taking out this, he introduces a pair of crooked scissars, enlarges the opening on each side by different snips, always as near as he can to the circumference of the *cornea transparens*, until he has made the opening round two thirds of the *cornea transparens*: He then takes out the scissars, and, with a small instrument like an ear-picker, he raises the *cornea*, and having in his right hand a cataract-needle, broader and stronger than the common, and pointed like a lancet, he cuts the *capsula* of the crystalline thro' the pupil; then, pressing gently the globe of the eye with his finger from below upwards, the crystalline slips out of the *capsula*, and drops out of the eye.

Upon the first puncture, the aqueous humour coming out, the *cornea* and *iris* join together: and it requires great dexterity, and a very steady hand, to introduce the instruments so as not to wound the *iris*, which would endanger the eye.

Tho' the operation lasted above two minutes, the patient, to my great surprize, never complained of any pain; and, upon my asking him, he said, he felt nothing but a tickling. By which it appears the *cornea* is not much more sensible than the nail of one's finger. And this operation, which seems so cruel to a by-stander, does not give so much pain as couching in the usual manner. It is to be preferr'd to couching in many respects. It may be performed at all times, and in all kinds of cataracts, whether they are come to maturity or not. Moreover one avoids many inconveniencies and accidents, which

often baffled the success of the best operations; such as the rising again of the cataract, violent defluxions and inflammations, which often destroyed the eye, the hurting of the vitreous humour, which seldom failed in couching, &c.

In both the operations, which I saw, the patient, immediately after, could distinguish all large objects in the room.

Paris, Sept. 25, 1752.

Read Dec. 11, 1752. **I**N regard to the remarks made by the skilful in your letter, he (M. Daviel) says, that he has found, by experience, that all those instruments are necessary: and as to the extent of the incision, he says, that he seldom makes it above one half of the circumference of the *cornea transparen-*  
*rens*; and that a smaller opening would not suffice to let the crystalline slip out easily; the diameter of which, in general, not being above a line less than that of the *cornea*, and, in some cases, within half a line, insomuch that, in order to make it pass thro' the *pupilla*, he has been obliged to give a snip of the scissors to the *iris*, which, he assures me, is attended with no bad consequences.

In answer to what is said, that it has been practised before, and that Taylor formerly performed it, he endeavours to prove, that it never was, excepting in cases where the crystalline had, by some accident, slipped thro' the *pupilla* into the anterior chamber.

In regard to the operation, there is some mention made of it among the Arabians, as what they had heard of; but the operation is not described particularly any-where. One convincing reason, that  
it

it never was carried into practice among the ancients, is, that, had they made the extraction of the cataracts, they must have found it to be the crystalline humour. and not remained in the error they have all fallen into, that the cataract was a membrane form'd in the aqueous humour.

In regard to Taylor, he may have attempted, but never did carry it into practice; else he would not have fail'd to have publish'd it in the numberless productions he has given. I know, that, in 1743, I follow'd him in Edinburgh for six months, where he performed above 100 operations of the cataract by couching; but never once attempted this way, nor ever mention'd it but in the case, where the crystalline is lodged in the anterior chamber; which operation has been described in many authors. So that I think Mr. Daviel may be truly said to be the first, who has brought this method into general practice for the cure of a cataract.

I think the greatest risk one runs in this operation is the pushing out of the humours of the iris thro' the opening, which forms a *staphyloma*; and I find this has been the case in some of those that have failed; and it is not easy to contrive a bandage upon that part, to make a compression equal to the resistance of the *cornea* before it was open'd. I am,

Dear Sir,

Yours, &c.

Tho. Hope.



XCI. *Letters of the Abbé Mazeas, F. R. S. to the Rev. Stephen Hales, D. D, F. R. S. concerning the Success of the late Experiments in France. Translated from the French by James Parsons, M. D. F. R. S.*

S I R, St. Germain, May 20, 1752, N. S.

Read May 28, 1752. **T**HE favour done me by the Royal Society obliging me to interest myself in whatsoever concerns their honour, I beg you will communicate the following account.

The Philadelphian experiments, that Mr. Collinson, a member of the Royal Society, was so kind as to communicate to the public, having been universally admired in France, the King desired to see them performed. Wherefore the Duke D'Ayen offer'd his Majesty his country-house at St. Germain, where M. de Lor, master of experimental philosophy, should put those of Philadelphia in execution. His Majesty saw them with great satisfaction, and greatly applauded Messieurs Franklin and Collinson. These applauses of his Majesty having excited in Messieurs de Buffon, D'Alibard, and De Lor, a desire of verifying the conjectures of Mr. Franklin, upon the analogy of thunder and electricity, they prepar'd themselves for making the experiments.

M. D'Alibard chose, for this purpose, a garden situated at Marly, where he placed upon an electrical body a pointed bar of iron, of 40 feet high. On the 10 of May, 20 minutes past 2 afternoon, a stormy cloud

cloud having passed over the place where the bar stood, those, that were appointed to observe it, drew near, and attracted from it sparks of fire, perceiving the same kind of commotions as in the common electrical experiments.

M. de Lor, sensible of the good success of this experiment, resolved to repeat it at his house in the Estrapade at Paris. He raised a bar of iron 99 feet high, placed upon a cake of resin, two feet square, and 3 inches thick. On the 18 of May, between 4 and 5 in the afternoon, a stormy cloud having passed over the bar, where it remain'd half an hour, he drew sparks from the bar. These sparks were like those of a gun, when, in the electrical experiments, the globe is only rubb'd by the cushion, and they produced the same noise, the same fire, and the same crackling. They drew the strongest sparks at the distance of 9 lines, while the rain, mingled with a little hail, fell from the cloud, without either thunder or lightning; this cloud being, according to all appearance, only the consequence of a storm, which happen'd elsewhere.

From this experiment we conjectur'd, that a bar of iron, placed in a high situation upon an electrical body, might attract the storm, and deprive the cloud of all its thunder. I do not doubt but the Royal Society has directed some of its members to pursue these experiments, and to push this analogy yet further.

I do not know, Sir, whether Mr. Franklin's letters were before your considerations upon earthquakes: if they were, we are oblig'd to Mr. Collinson for his communication of Mr. Franklin's notions; if they are not, you deserve the honour of the discovery; and whose-

soever it be, it is still to the Royal Society we owe the communication of this ingenious thought, which the experiments of M. D'Alibard and M. De Lor have confirm'd. These two learned men deserve that esteem of our nation, which their talents have a long time procured them. I am, with a profound respect,

S I R,

Your most humble, and

obedient servant,

G. Mazeas.

S I R,

St. Germain's, June 14, 1752.

Read Nov. 23,  
1752.

**M**ONSIEUR D'Alibard, the translator of Mr. Franklin's treatise relating to electricity, acknowledges, that the ingenious discovery of the analogy between thunder and electrical matter is due to you. Since you were the first \*, who gave us a clear idea of it, I ought to not be wanting to give you an account of the advances, which this discovery has made in this country.

On

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\* In this the Abbé Mazeas has not been sufficiently well informed, as, for several years, this analogy has been deduced by several gentlemen of the Royal Society, who were engaged in these pursuits. Even the late Mr. Stephen Gray, so early as the year 1735, takes notice of it, and says, "that this electric fire, by several of these experiments, seems to be of the same nature with that of thunder and lightning." See *Phil. Trans.* N. 436.

On the 7 of June, a violent storm happening at Paris, and about it, the greater part of the philosophers endeavour'd to repeat the experiment, which I had the honour to mention in my last letter. I was assured, that no one succeeded at Paris; some looking upon the experiment as false, while others attributed their want of success to the abundance of rain, that wet the cakes of resin, which they used to support the bar of iron.

M. Le Monnier, having prepared to repeat the same experiment here, in the presence of the Duke D'Ayen, avoided that inconvenience in the resin cakes. He placed, in the garden of the *hôtel de Noailles*, a wooden pole, of about 30 feet high, at the end of which was fix'd a large glass tube, which receiv'd at the other end a long tin pipe; and this pipe receiv'd again, in its turn, a pointed bar of iron, of about 6 feet high. The glass tube, as you see, was instead of the cake of resin, to hinder the communication of the electricity from the tin pipe to the pole. A wire was carried from the bar of iron, which rested upon a filken cord, about 50 paces from the pole; but rain coming on, the wire was conducted into the house. We perceived the commotions of the electrical matter from the first clap of thunder; it produced sparks, and there were certain intervals, wherein the commotions were so strong, that they were accompanied with very sharp pain: and I am persuaded, that, if the tin-pipe had triple or quadruple more surface, no one could touch the bar of iron, without paying dearly for it. It seem'd to me, as if the commotion was the greater, the nearer the thunder was to the bar. This is the experiment, that was executed

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here, which I was a witness to. The fear, that seiz'd several ladies, who were present, hinder'd its continuation; and we were even obliged to take away the bar, and the whole apparatus.

After this experiment, I propos'd satisfying myself concerning a notion I conceiv'd, and which the weather suffer'd me to execute but imperfectly. The nature of the vapours, which compose thunder, is not absolutely unknown to us. Would not the mixture of salts, sulphur, pyrites, &c. produce vapours capable of electrifying a bar of iron? By suspending a bar of iron upon silken cords, and causing a wire to descend into a large glass recipient, wherein pyrites and other analogous matters, as sea-salt with oil of vitriol, may be made to ferment, in order to produce a vapour, which would contain spirit of salt, or which might develope the electrical matter; might not we come to produce the same phenomenon with that produced in a storm? Upon this footing I tried some experiments, which my business hinder'd me from pursuing; but the success did not perfectly answer my expectation. I thought I perceiv'd some signs of electricity; but they were so doubtful, that I do not mention them. If I make any future attempts of this kind, I shall have the honour of communicating them. I am, with the most respectful attachment,

S I R,

Your most humble and

most obedient servant,

Guill. Mazeas.

S I R,

S I R,

St. Germain, June 29, 1752.

Read Nov. 23  
1752.

ON the 26 of this month we had a storm at two different times; the first was at 3 in the afternoon, and the second at half an hour after 6. This storm, which came from the south-west, was very inconsiderable: there were but two or three claps of thunder, either at 3 or at 6 o'clock; and there was a considerable interval between the lightning and the clap, which shew'd, that the thunder was at a great distance. Nevertheless the effects of the electricity were very violent, which I attribute to M. Le Monnier's ingenious apparatus; which is as follows:

It is certain, by M. Muschenbroek's experiments, that the more surface the electrified bars have, the commotions are the more violent; but, as it would be difficult to fasten rods or bars of a certain size to the ends of the great wooden poles, M. Le Monnier has ingeniously supplied that defect, in forming a magazine for the electricity. This magazine is only a communication of the electricity, which descends from the bar of iron, situated at the top of the pole, with several other large bars of iron placed near the pole.

The greater the quantity of these bars, the greater is the quantity of electricity furnish'd by the magazine.

In the last experiment we had a tin pipe, of 7 feet long, and about 5 inches diameter. It was the first magazine: the second consisted of six great bars of iron of six feet long each, placed in parallel order upon glass bottles. All these magazines communicated with the iron wire, that descended from the little bar at the top of the great pole, which I described in my last letter.

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The

The 26 of this month, at 3 afternoon, very lively sparks were excited, and M. Le Monnier set fire to spirits of wine. At 6 o' clock I went up to a proper place, in order strictly to observe the intervals between the commotions and the electricity.

The clouds extended from the south and west to the zenith of the pole, and the lightning came from a very distant part; and, in proportion as the clouds came nearer, the electricity was felt with very smart shocks, but without light, or regularity; for sometimes none were felt for two or three minutes; and it was commonly with every flash of lightning that the commotion was felt. But when the clouds had cover'd a considerable part of the heavens, the commotions of the electricity succeeded very quickly with noise and sparks; altho' the thunder could scarce be heard, because of its distance. It may from hence be judged, how strong the commotions would be, if the clouds, which produced the thunder, were nearer the bar.

On the 29 of June we had another storm; but I was not present at the experiments made in the garden, being myself employ'd in a like experiment in my chamber. I placed at my window, which was about 35 feet from the ground, a bar of iron of 12 feet long, which receiv'd a very sharp iron wire of six feet high; the whole advanced into the street, by means of a wooden pole laid parallel to the horizon; at the end of which was a glass tube fill'd with resin, in order to receive the iron rod. The wire, that hung from the extremity of the pole, enter'd into my chamber, and from thence into a gallery of 30 feet long. The electrical magazine was in my chamber,

chamber, and the iron wire, after several turnings, was again brought thither. I had disposed of this wire in such a manner, that, if the storm should come in the night, or if it happen'd by day, I had it in my power to observe all I propos'd, without quitting my bed on the one hand, or leaving my business on the other.

The storm came at 5 in the evening; and although I had not yet time enough to form a sufficient magazine of electricity, I had nevertheless very satisfactory signs. The person, who held the iron wire, felt a commotion; and, at the same instant, silken ribands were attracted by the electrical magazine. There came on a great shower of rain and hail, which wetted the resin in the glass tube, that supported my bar; and after that I had no more signs of electricity.

The same thing happen'd in the garden; where the silken cords, which, in several places, interrupted the communication of the electrified bodies with the non-electrics, having been wet, sensibly diminished the desired effect. The electricity, however, was very strong before the rain fell; and the commotions were felt at about a foot distance: but the storm only passed by, and lasted no more in the whole than two or three minutes.

Hence, Sir, it follows, that the electrical magazine is an important object in experiments of this kind. I do not even doubt, but that, by placing guns and bars of iron, in great numbers, in places adjacent to the wooden pole, we might even come to kill animals, and verify all the surprising phenomena, that thunder has produced for so many ages.

This



This may be done without going out of one's room, and even in bed, where one might easily be assured of the degree of the force of a storm, by the degree of the strength of the commotions: and if we were loth to touch the iron wire with a finger, for fear of the pain, we might use a little plate or blade of tin, fastened to the end of a glass tube. One might, by this means, have the satisfaction of judging of the degree of the strength of thunder.

I forgot to observe, that my iron bar was too near the neighbouring houses, which greatly lessen'd the electrical power.

I beg, Sir, you will, in my name, assure the Royal Society of the sentiments of respect and acknowledgement, which I owe it, for the honour it has done me. I also repeat them to you, Sir, and to Messieurs Wilson, Pringle, and Knight; and am,

S I R, &c.

Guill. Mazeas.

S I R, St. Germain, July 12, 1752.

Read Nov. 23, 1752. ON the first, second, and tenth of July, we had storms at St. Germain; of which I have the honour to give you an account.

I was not a witness to the experiments, that were made on the first and second of this month in the garden of the Hôtel de Noailles; because I was then busy in my chamber; which I shall mention by-and-by: but the following is what was told me, and

and since confirm'd, by Mr. Le Monnier, who perform'd them himself.

1. He was convinced, that the high situation, in which the bar of iron was commonly placed, is not absolutely necessary to produce the effects of electricity: for a tin speaking trumpet suspended upon silken cords about five or six feet from the ground, has produced very particular signs of electricity.

2. A man, placed upon a cake of resin, and holding with his hand a wooden pole, of about 18 feet long, round which an iron wire was twisted, was so well electrified, while it thunder'd, that sparks, which were very lively, were drawn from his face and hands.

3. Having taken away the communication of the electrical magazine with the iron wire, which hung from the great wooden pole (this magazine consisted, as I have said in my last letter, of 6 great bars of iron, placed horizontally upon glass bottles, about 4 feet from the ground) I say, this magazine was strongly electrified, when the stormy cloud passed in the zenith.

4. A man, standing upon the electrical cake in the middle of the garden, and simply holding up one of his hands in the air, attracted with the other hand wood-shavings, which were held to him upon a piece of lead. Whence it evidently follows, that the matter, which is the cause of all the surprising phenomena, which electricity affords us, fills the atmosphere in the time of a storm; that it penetrates us; that we breathe it with the air; and that the height usually given to the iron bar only serves to intercept the far greater quantity of the electrical matter.

At the time that Mr. Le Monnier made his experiments, I, in my turn, tried to perfect the manner of bringing the electricity into my chamber. This method seem'd to me the more essential, as the glass tubes, which Mr. Le Monnier substituted to the electrical cakes have not the advantage of keeping the electricity in the iron bar, when a good deal of rain falls. When these tubes are too wet, the electricity ceases.

I therefore increased the length of my wooden pole, which went out of my window, and, at the same time, that of my iron rod, which was perpendicularly fastened to its end. The greater the length and height of these two were, the stronger was the electricity in my chamber; which led me to the two following observations:

1. My chamber having two windows opposite to each other, the one to the south, looking into a street, and over-against the neighbouring houses; the other to the north, with an unbounded prospect of the country; I found the electricity was stronger, when my pole was supported by the resin cake placed upon the north window, than in the other opposite to the houses; which made me imagine, that the electrical matter was more strongly attracted by the neighbouring large buildings than by my pole.

2. I observed a considerable diminution of the electricity when rain came on, altho' the thunder roar'd very strongly, and the cake of resin on my window was not wet: which made me think the rain, as it fell, might deprive the atmosphere of the electrical matter, when it is in a sufficient quantity to carry away with it a large portion of that matter

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Here is a fact, which establishes that opinion : when the rain ceased for some time, my pole, altho' wet, produced new signs of electricity.

Hitherto the electricity appear'd to me to be stronger in the beginning, than in the middle or end of a storm ; that is, in proportion as it approach'd, till it was immediately over the pole.

I draw this fact from the observations, which I made from the first and second of July, without giving it as a general one : however, I now reassume the experiment of the 20 of the same month.

Towards 11 in the morning, the heavens began to be cover'd to the south-west, with some claps of thunder and lightning at a great distance. I had just time to go to the garden, where I found the Duke d'Ayen, who had prepared every thing for the experiments. An iron wire descended from the top of the pole, and rested upon the hot-house of the garden : this wire was supported by a silken cord, and was terminated by a tin cylinder, of about 3 inches diameter, and 3 feet long. The electricity of this cylinder was such, that, when a finger approach'd it, two or three very lively sparks at a time were produced, with a sparkling noise, like that of the nails of one's fingers crackled against each other.

Then the Duke d'Ayen took the first shrub he met in the hot-house, which happen'd to be that, from which the *labdanum* is produced, as well as I can remember : he placed it with its pot on a cake of resin, and fastened the iron wire to one of its branches. This shrub was instantly electrified, so that whitish sparks issued from every leaf, with the same kind of crackling I have just mention'd ; but

It remains, Sir, that I communicate to you the observations, that Mr. Ludolf made at Berlin. I present you with them, as Mr. Euler was so kind to send them, which I have transcribed word for word.

“ As I was not present (says this learned man) at  
 “ the experiments made upon thunder, I will have  
 “ the honour to transcribe for you the recital, that  
 “ Mr. Ludolf communicated to me. The experiments  
 “ were made the 19 and 26 of July, and the 1 and  
 “ 2 of August;” and it is observed,

1. That the sparks drawn from the wire were half an inch long; and they caused so horrible a shock, that the intire body of the person, who attracted them, was shaken; but the finall sparks produced only a light sensation in the fingers.

2. It is also remark'd, that this electricity communicates itself to all bodies elsewhere, that are susceptible of it, provided they are placed upon electrical bodies, while they are made to communicate by a wire.

3. When there was plenty of rain, we scarce remarked any thing of the force of the electricity, altho' the lightning and claps of thunder were very strong.

4. At every clap of thunder the electricity seem'd extinct, and returned not till after 30 seconds, or thereabout, and sometimes longer.

5. When the wire was furrounded with drops of rain, it was observ'd, that only some of them were electrical, which was remarkable by the conic figure they had; whilst the others remain'd round as before. It was also perceived, that the electrical and non-electrical drops succeeded almost alternately;  
 which

which made us call to mind a very singular phænomenon, which happen'd some years ago to five peasants, who pass'd thro' a corn-field near Francfort upon the Oder in a storm. The thunder kill'd the first, the third, and the fifth, without injuring the second and fourth.

6. The storm of the first of August was very considerable, with very great rain; every minute we remarked 3 or more flashes of lightning; in the mean time some electrical sparks were observ'd upon the wire. They put upon a chain, which communicated with the wire, a thread, the two ends of which hung down; which shew'd electricity, by mutually repelling each other; for, at every flash of lightning, they approached each other suddenly, as if they had been push'd one against the other by some force.

7. Sometimes the electricity continued in the wire with great strength to 45 minutes, after the thunder and lightning had intirely ceased, &c.

Conformable to the 6 observation of Mr. Ludolf, I have often observed, that, in presenting dust or dry'd snuff to the end of a tin cylinder, which hung to the wire in these sort of experiments, this dust was strongly attracted, as soon as the wire shew'd any signs of electricity. But, when the electrical matter came to be accumulated in this cylinder, the dust was powerfully repell'd as by a strong blast, insomuch that the quantity of molecules repell'd was much greater than of those attracted at the same time.

And with respect to this successive attraction and repulsion, I must not pass by in silence an experiment

I was

wire of about 20 feet long came down, and rested upon a long glass tube fixed to the balustrade, which environ'd the gallery. My apparatus was scarce ready, when it thunder'd, and the clouds broke by this first clap, and pour'd down a continual large quantity of rain, which lasted near 2 hours, without the least discontinuance of the thunder.

I felt no commotion in putting my finger towards the wire, nor could I draw any sparks from it. I was upon the point of giving it over, when the wire happen'd to touch the leads and the balustrade of the gallery; and it instantly produced as many sparks, as it touch'd places on the balustrade and leads. I then took the wire in my hand, and threw it strongly against the bars of iron; and as the wire extended, and successively touch'd the bars, it always produced the same effect. There were prodigious multitudes of these shining sparks, like those produced by the finger in common experiments. I only wanted an electrical magazine to accumulate electrical matter in, which would have produced me all the usual phænomena. The thunder was in its greatest vigour from half an hour after 8 to half an hour after 9; during which the rain was most abundant, and I repeated my experiment at several times.

It is therefore certain, 1. That the electricity sometimes ceases when it rains, but not always; because, in the present case, the wire was as much impregnated with the electrical streams as it could be.

2. That the first and second question proposed above do not include the true cause of the cessation of the electricity at the time of rain; since there are few storms, in which the rain is more abundant than

than this which fell the 12 of July in the evening, and wherein my apparatus was as wet as it could be.

3. It is again certain, by Mr. Ludolf's third experiment, mention'd hereafter, that this cessation does not happen, because the matter of the thunder is extinct. " When the rain was abundant (says he) we " remark'd nothing of this force of electricity, altho' " the lightning and the claps of thunder were ex- " ceeding strong."

The true cause of these kind of diminutions may therefore depend on some other principles, which we have not as yet come to the knowledge of. Hitherto this phenomenon presents us with a great many variations. I have seen circumstances, wherein simple clouds, without thunder or lightning, produced more electricity than when there was loud thunder: I have seen others, wherein the electricity did not shew itself but where there was lightning; and, in short, others, when the electricity, which seem'd dissipated during the rain, began again as soon as the rain ceased, altho' the thunder was very distant. The few experiments hitherto made are not sufficient to pronounce any certain opinion upon, with so many variations.

The little success I have had in trying, whether strong explosions, or violent fermentations of salts, sulphurs, and several liquors, would not produce some signs of electricity, does not surprize me. The matter contained in clouds may be of a different nature. The atmosphere of the earth is a more powerful sublimator than those of our chemists; and our weak operations will never perfectly come up to those of nature.



the trunk of this shrub had a much stronger electricity ; whether, at that instant, the electricity of the cloud was more strong, (for it varies every moment) or that the force of the whole electricity, expanded thro' the leaves, became concentrated in the trunk of this shrub \*.

The Duke then took one of his silver watering-pots, which was two feet and an half high ; he fill'd it with water within an inch of the brim, and placed it upon the electrical cake, dipping into it a wire of lead, which communicated with that wire, which came from the top of the pole. Of all the electricity tried till then, this was incomparably the strongest : nor did I see any sparks, when I advanced my finger towards it, but the shock affected me in the arms and breast with such violence, that I did not attempt to make a second trial. Wherefore it would be well, before one runs the hazard of such sort of experiments, to try the force of the electricity, by applying an iron wire, or a piece of steel, fastened to a little glass tube. During these experiments M. le Monnier was absent ; which deprived us of some new designs, which he had resolv'd to put in practice.

I am, Sir, &c.

G. Mazcas.

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\* This experiment was made the 2 of July by M. le Monnier, and repeated, as I have now mention'd it, on the 11 of the same month.

S I R,

Paris, Aug. 21, 1752.

Read Nov. 23,  
1752.

**A** Phænomenon, which I have always thought worthy of strict observation, is the diminution of the electricity of thunder, when rain comes on during the storm. This diminution was remarked at St. Germain, every time I was a witness to M. le Monnier's experiments; and the same effect is, within this little while, confirm'd to me by the learned Mr. Euler, in communicating to me the observations of M. Ludolf. I have thought of only three causes assignable to this phænomenon, which I lay down in the following order:

1. Does not this diminution happen, because the drops of water, that run down the little bar of iron, carry with them the electricity of the bar?

2. Does not the rain, in passing thro' the atmosphere, deprive or strip it of the electricity, which is communicated to it by the thunder?

3. Or else, is it not more likely, that the diminution, and the total cessation, of the electrical streams happen then, either because the matter of the thunder is exhausted, or because the clouds coming to dissolve, the electrical matter is lost and dissipated?

I left St. Germain the 12 of July to come to Paris, at 7 in the evening. At the instant of my arrival, I saw the heavens cover'd with clouds, and the lightning foreboded thunder, which soon was heard. I went up into the gallery of the Hôtel de Noailles, which is very high, and distant from the neighbouring buildings: my pole was 10 feet high; at the end of which a glass tube was made fast; and to this a very sharp iron spire, from the middle of which a

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wire

I was informed of, without knowing the author of it \*. The dishes of a pair of scales were suspended to the balance by silken cords; the two dishes were electrified, and a very sharp needle was presented to one of them. The scales immediately lost their equilibrium; and that dish, under which the needle was held, was attracted. The direct contrary happen'd, when an obtuse or round body, such as a leaden bullet, was put upon the point of the needle, for then the dish was repell'd.

If this experiment be true, as I have all the reason in the world to believe it so, it strongly imitates what happens in the clouds, when they are *in æquilibrio* in the atmosphere: and it gives us room to conjecture, that it would be much less dangerous to terminate the tops of steeples with obtuse bodies, than with pointed spires, upon which the thunder falls sooner or later when they are very high.

As the year begins to draw to an end, I believe these observations will be the last for the year 1752. an epocha, which will always be famous with the lovers of electricity; and particularly myself, because it has given me an opportunity of testifying from time to time the respect I have for your person, and the acknowledgments I owe to that friendship, with which you honour

Your most humble, &c.

G. Mazcas.

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\* Since I wrote to Dr. Hales, I found this experiment among those of Mr. Franklin.

XCII. *Extraits of Two Letters of the Abbé Nollet, F. R. S. to Mr. William Watſon, F. R. S. relating to the extracting Electricity from the Clouds. Translated from the French.*

Paris, June 6. 1752. N. S.

Read June 11, 1752. **T**HE Abbé, after having taken notice of the discovery of M. Dalibard in France, in relation to the extracting the electricity from the clouds during a thunder-storm, in consequence of Mr. Franklin's hypothesis, acquaints Mr. Watſon, that he is more interested than any body to come at the facts, which prove a true analogy between lightning and electricity; since these experiments establish incontestably a truth, which he had conceived, and which he ventured to lay before the public more than four years ago. Examine but the fourth volume of his *Leçons de Physique*, pag. 314, and you will find what follows: "If any one should  
 " take upon him to prove, from a well-connected  
 " comparison of phænomena, that thunder is in the  
 " hands of nature, what electricity is in ours; that  
 " the wonders, which we now exhibit at our pleasure, are little imitations of those great effects  
 " which frighten us; and that the whole depends  
 " upon the same mechanism; if it is to be demonstrated, that a cloud, prepared by the action of the  
 " winds, by heat, by a mixture of exhalations, &c.  
 " is opposite to a terrestrial object; that this is the  
 " electrified body, and at a certain proximity from  
 A a a a " that

“ that which is not; I avow, that this idea, if it was  
 “ well supported, would give me a great deal of  
 “ pleasure; and, in support of it, how many spe-  
 “ cious reasons present themselves to a man, who  
 “ is well acquainted with electricity! The univer-  
 “ sality of the electric matter, the readiness of its  
 “ action, its inflammability, and its activity in giv-  
 “ ing fire to other bodies; its property in striking  
 “ bodies externally and internally, even to their  
 “ smallest parts; the remarkable example we have  
 “ of this effect in the experiment of Leyden; the  
 “ idea, which we might truly adopt in supposing a  
 “ greater degree of electric power, &c. all these  
 “ points of analogy, which I have been some time  
 “ meditating, begin to make me believe, that one  
 “ might, by taking electricity for the model, form  
 “ to one’s self, in relation to thunder and lightning,  
 “ more perfect and more probable ideas, than what  
 “ have been offer’d hitherto, &c.”

To demonstrate, that glass is not absolutely impermeable to the electric fluid, I offer the following experiment:

Let the neck of a small thin phial *A* (see the Fig.) be placed in that of the receiver *B*; and lute it in such a manner, as that the air cannot pass through their joining. Exhaust the receiver, and pour the little phial three parts full of water, and conduct the electricity therein, by means of an iron wire, suspended to the conductor. Make the experiment in a dark place, and, for the greater surety, fix the receiver to the plate of the air-pump, not with wet leathers, as usual, but with soft cement. You will see the electric matter pass, as through a sieve, through the small phial into the receiver, and  
 present

present itself in an infinite number of luminous streams, of extraordinary beauty ; and, if you do not take care, you will be smartly shocked, as in the experiment of Leyden, by laying one hand upon the receiver, and touching with the other the plate of the air-pump.

To prove, that, in the experiment of Leyden, the electrical virtue, or power of giving a shock, does not reside *only* in the glass, make the following experiment :

Electrify a phial two thirds full of water ; pour this water into another thin phial, placed upon a glass stand ; plunge therein an iron wire, and attempt, while the phial is in one hand, to draw a spark with the other ; it is certain, that, if this is done with a little readiness, you will make the experiment of Leyden with this water §. Possibly you may not always succeed with water ; but with mercury, under the same treatment, it never fails. Whence proceeds the power of giving the shock to the second glass, if it is not by means of the water, which it has received ?

Electrify a bolt-head of glass, void of air, and sealed hermetically ; you may make use of it for the experiment of Leyden, and you will succeed. Is

A a a a 2

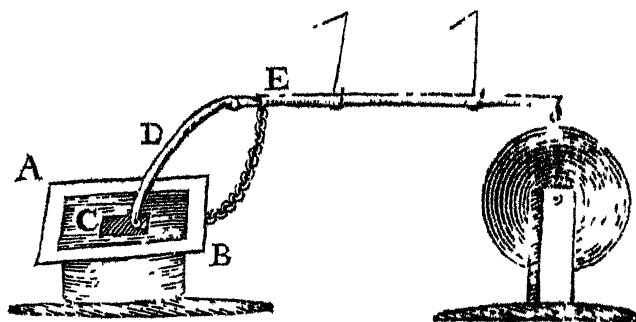
there

§ Some years ago I shewed this experiment to several members of the Royal Society, and did not only therewith produce the experiment of Leyden, but by pouring the electrified water into a basin, held in one hand of an assistant standing upon cakes of wax, who, upon his presenting a finger of his other hand to some warm spirit of wine in a spoon, held in the hand of a person standing upon the floor, set it on fire. I then consider'd this experiment, as a proof of the electricity being accumulated in the water.

W. Watson.

there not then a communication between the exterior and interior surface of the glass? And is it not evident, further, that the electric matter, which is perceived running within like a torrent of fire, passes through the glass?

When you force a hole through a piece of paper or pasteboard, attend to one thing, which I constantly observe. If you electrify the plate of glass, *AB*, underneath, and that, by means of a thick iron wire somewhat bent, *D*, you draw the spark thro' a piece of pasteboard, *C*, placed upon the metal, with which the glass is coated, the hole will appear invariably larger underneath, than on the top of the pasteboard; and this hole will have an impression at the place, where the iron wire shall have been supported. These two effects leave no room to doubt, but that the stroke of fire was directed from the glass to the conductor, *F*, by the bent iron wire. Besides, if the electric fire proceeds from the upper surface of the glass, which receives the electricity from the under surface, it necessarily follows, that it must have passed through the whole thickness of the plate of glass; and, consequently, that the glass is not impermeable to the electric fluid.



Paris, July 22, 1752.

Read Dec. 14, 1752. **T**HE electrical experiments, which have been made here during the thunder, are now sufficiently verified. Dr. Le Monnier, assisted by his advantageous situation, has sufficiently experienced, first, that a bar of iron, pointed or not, is electrified during a storm: Secondly, that a vertical or horizontal situation is equally fitting for these experiments: Thirdly, that even wood is electrified: Fourthly, that, by these means a man may be sufficiently electrified to set fire to spirit of wine with his finger, and repeat almost all the usual experiments of artificial electricity; for thus I denominate that, which is excited by friction.

Seeing, therefore, that these experiments succeeded so well, I attempted them at Paris with a tube of tin, eighteen feet in length, and of an inch and half in diameter; half of which tube I put out of my window, while the other half was placed upon, and fastened to, silk lines: And though I live in the lowest part of Paris, and my apartment in the Louvre is covered with an immense building, both in height and extent, at any time when the thunder was but moderate, I perceived therefrom signs of electricity. The sparks were more frequent after the lightning than after the thunder; and it even seemed, that the clap of thunder put a stop, for a very short time, to the force of the electricity.

Monf. Cassini de Thury, who was desirous of observing these effects with the apparatus, which we had erected upon the terrace of the observatory, made the same remarks; and he has had a greater opportunity



tunity of observing them, because the effects there were more considerable than at my apartment, on account of the situation. He even remarked to-day very evident signs of electricity, although there was neither lightning nor thunder, but only the sky cover'd with such thick clouds, as seemed to forebode a storm.

Mons. Le Roy, a member of the Academy of Sciences, who lives near me, has repeated also a great number of these experiments and observations, by only making use of a pole of wood twenty-five feet long, about which he turned an iron wire in form of a screw.

This, Sir, is the state of these matters with us at present, which I am very far from thinking that we are arrived at the complete knowledge of. I have reasons for suspecting, that there frequently happens a natural electricity in the atmosphere. It may be, that thunder is only a circumstance, and not the efficient cause, of all these effects, which now present themselves to us; and it is not impossible, but that the great mystery of vegetation has great connection with this natural electricity. Time and observations may throw some light upon these important questions.

*XCIII. Extract of a Letter from Mr. Mylius  
of Berlin, to Mr. W. Watson, F. R. S.  
upon the before-mention'd Subject; dated at  
Berlin, August 26, 1752.*

Read Dec. 14, 1752. **M**ARCH 16 past, at a little past 8 in the evening, we had here at Berlin a slight earthquake, which manifested itself by its shaking the ground, the windows, and by opening some doors. This before we have had no example of in our country; and it was perceived at the same time at Stavanger in Norway. I have made experiments of collecting the electricity, during a thunder-storm, with great success, in company with Professor Ludolf. He had erected an iron bar, of twelve feet long, which was fasten'd upon a pole of wood, fifty feet in height, with two tubes of glass cover'd with tin. The upper end of the iron bar was sharp-pointed, and near the lower end was fastened a very long iron wire, which being carried into a summer-house, gave great sparks, as the thunder was approaching; and these sparks caused sometimes as violent a shock through the body, as the experiment of Leyden. It was also continually observed, that the effects were greatest, when the lightning was nearest; and that, for some moments after the lightning, the effect ceased, but returned and increased by degrees.

XCIV. *Mons. Faget's Remarks on the Use, &c. of the Styptic, purchased by His Most Christian Majesty; communicated by James Theobald, Esq; F. R. S.*

Read Dec. 7,  
1752.

**A**BOUT the end of the year seventeen hundred and fifty, Mr. Broffard, a surgeon from Berry, came to Paris, to propose the use of a remedy, which he had discover'd for stopping the blood after amputations, and which he asserted to have found effectual in several amputations of the arms and legs. At his request, some gentlemen of the Academy of Surgery were deputed, in whose presence he was to make some new experiments in stopping the blood upon some of the animals, and in all which he succeeded, by stopping it in the largest arteries after amputation. But the success of this remedy might yet be consider'd a little dubious, because in many animals, as in dogs particularly, the great arteries stop of their own accord; and rarely any dog dies from an hamorrhage, because their blood is more disposed to congeal, and by that means stop the discharge.

For this reason the experiments made on animals not being thought satisfactory, and yet being convinced, ~~that~~ no ill effect could follow the application of this remedy on human kind, Mr. Broffard was permitted to use it at the hospital of the invalids, in an amputation of the leg, which succeeded perfectly well; and not the least ill accident attended the cure thro' the whole time.

Some

Some time after this two waggoners were run over by a waggon loaded with stone, and each of them had one leg broken in a miserable manner. These two men being brought to the Hospital of the Charity, I saw no other hopes of success but in amputating the legs; and, for that reason, I requested Mr. Broffard would be present, and give me a proof of this new application, which we applied in the following manner:

As soon as the leg was cut off, I slacken'd the tournequet, to discover the vessels; and Mr. Broffard applied, upon the orifices of the two arteries, two pieces of his astringent, fasten'd one upon the other with a riband, in the manner, which I have sent to you, and as it is in the drawing. After the application was made, I streighten'd the tournequet, and pass'd the two ends of the riband, which was fasten'd to the upper piece of the astringent, upon the stump over the knee, and applied a linen bag, filled slightly with the same astringent in powder, upon the whole wound; and, over all, applied the common dressings in the like case. After the dressing was finish'd, I slacken'd the tournequet, and two hours after took it intirely away. Eight-and-forty hours after this, we took off the dressings, and not the least drop of blood follow'd from the vessels: and we again applied one single piece of the astringent upon the two vessels; and I dress'd the other parts of the wound with pledgets of lint, with common digestive, a styrax plaster, and the usual bandage.

The third day the astringent fell off of itself in the time of dressing; and the patient, after that time, was

B b b b

dress'd

dress'd in the common manner. The same was done to the other patient, after the amputation, as to this.

The first of these men died on the fifth day, and the other on the ninth: but there did not appear, thro' the whole, the least tendency to an hæmorrhage. Thus the remedy fairly produced its effects, as to the stopping the blood.

However, in order to determine the manner, in which this astringent produces its effect, I examined the blood-vessels of those two patients after their death, and I found them contracted and straiten'd, as if they had been tied, and in the largest of them a conic coagulation of the blood, which was an inch and half long: and after having taken out this coagulation, it was with difficulty, that I could introduce the point of a very small probe into the orifice of that vessel.

The patient, who died on the ninth day, had the arteries contracted in the same manner; but with this difference, that the congelation was at least four inches long.

Mr. Morand has employed this remedy with success, in applying it to a wound, made by a sword, in the bending of the arm: and I myself have made use of it, with great success, on occasions, where the temporal and intercostal arteries have been open'd.

In the last-mention'd cases, I applied but one piece of the styptic upon the opening of the artery; and this generally falls off at the first dressing, that is, forty-eight hours after the application, without the least appearance of an hæmorrhage, or other ill symptoms, which can raise any objections to this styptic; for those patients are all recover'd.

There

There have been lately made, at the hospital of the invalids, two experiments of this astringent in amputations; and in both the success has been equal to all that can be desired. The surgeon, in these cases, used only the two pieces applied one upon the other, without using the powder in the bag, as before; and dress'd the whole wound with lint, and the common bandage.

Thus, then, at last there appears to be discovered a remedy beyond our hopes, and which art has never yet equall'd. The application of fire was the cruel resource of the antients; and Paré believed himself inspired, when he discovered the use of the ligature. But, alas! how many accidents are there, which arise from the use of those two manners, and which too often terminate in the death of the patient! Happy for us, that those accidents now appear to be no longer to be fear'd by the lucky discovery of this styptic, the first experiments of which have so greatly promised success!

It may be remarked, that, if this astringent succeeded only in coagulating the blood, it had produced nothing extraordinary; for these coagulations would not have been sufficient to have stopp'd the hemorrhage, directly after the operation in amputations: but its excellency lies in contracting the arteries so closely, that it hardly lets a little probe into the aperture of the artery, and by this means forms, as it were, a perfect ligature, much more certain than the usual one; as this is not made in any one point of the cylinder of a vessel. Thus this application exceeds every thing, which has hitherto been produced by the operation of our hands.

This singularity in the operation of this remedy supposes another in the vessels, which is the great contractility of the fibres of the arteries. These, indeed, do naturally contract of themselves; but not to two thirds of their diameter; nor to that state, in which they are straiten'd by the effect of this astringent; because, by that, the whole aperture is almost intirely taken off in the largest vessels; and it is easy to imagine their effects in the smallest.

It may be observed, that it is not in the dead parts of bodies, that this contraction can be made: it requires the assistance of the vital principle, and operates on the fibres by certain articles contained in it, which dispose the animal body, by its irritation, to shorten its fibres, and reduce the tissue, which they compose, into a lesser volume.

This remedy, of which I have been speaking, is nothing else but the agaric of the oak. The best kind of it is found on the parts of oak-trees, where the large limbs have been cut off; and it very often resembles a horse-shoe in its shape. This agaric is distinguished into four parts; the rind; the second part, which is preferable to the other; the third part serves for the stopping the blood in the smaller vessels, as well as that part, which touches the tree. This last was what was powder'd, and applied in the little bag, as in the operations of the Charity.

The second part is what I make use of in amputations, which is cut into pieces, of the size of that which I have sent you. It must be beaten by a hammer till it is soft; and this is its whole preparation. Every part is prepared alike.

The

The best time of collecting it Mr. Broffard has found to be in the autumn, in fine weather, after great heats.

This, then, Sir, is all I can collect of the use, application, and preparation of this new remedy for stopping blood. If the Royal Society shall find any thing in it worthy their regard, I shall think myself happy in having communicated these observations. I am

Your most obedient servant,

Faget.

*XCV. A Letter of Benjamin Franklin, Esq;  
to Mr. Peter Collinson, F. R. S. concerning  
an electrical Kite.*

Philadelphia, Oct. 1, 1752.

Read Dec. 21  
1752.

**A**S frequent mention is made in the public papers from Europe of the success of the Philadelphia experiment for drawing the electric fire from clouds by means of pointed rods of iron erected on high buildings, &c. it may be agreeable to the curious to be informed, that the same experiment has succeeded in Philadelphia, tho' made in a different and more easy manner, which any one may try, as follows :

Make a small cross, of two light strips of cedar ; the arms so long, as to reach to the four corners of a large thin silk handkerchief, when extended : tie the corners of the handkerchief to the extremities of the cross ; so you have the body of a kite ; which  
being



being properly accommodated with a tail, loop, and string, will rise in the air like those made of paper ; but this, being of silk, is fitter to bear the wet and wind of a thunder-gust without tearing.

To the top of the upright stick of the cross is to be fixed a very sharp-pointed wire, rising a foot or more above the wood.

To the end of the twine, next the hand, is to be tied a silk riband ; and where the twine and silk join, a key may be fasten'd.

The kite is to be raised, when a thunder-gust appears to be coming on, (which is very frequent in this country) and the person, who holds the string, must stand within a door, or window, or under some cover, so that the silk riband may not be wet ; and care must be taken, that the twine does not touch the frame of the door or window.

As soon as any of the thunder-clouds come over the kite, the pointed wire will draw the electric fire from them ; and the kite, with all the twine, will be electrified ; and the loose filaments of the twine will stand out every way, and be attracted by an approaching finger.

When the rain has wet the kite and twine, so that it can conduct the electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle.

At this key the phial may be charged ; and from electric fire thus obtain'd spirits may be kindled, and all the other electrical experiments be performed, which are usually done by the help of a rubbed glass globe or tube, and thereby the sameness of the electric

tric matter with that of lightning completely demonstrated.

I was pleased to hear of the success of my experiments in France, and that they there begin to erect points upon their buildings. We had before placed them upon our academy and state-house spires.

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XCVI. *A Letter of Mr. W. Watson, F. R. S. to the Royal Society, concerning the electrical Experiments in England upon Thunder-Clouds.*

To the Royal Society.

Gentlemen,

Read Dec. 21,  
1752.

**A**FTER the communications, which we have received from several of our correspondents in different parts of the continent, acquainting us with the success of their experiments last summer, in endeavouring to extract the electricity from the atmosphere during a thunder-storm, in consequence of Mr. Franklin's hypothesis, it may be thought extraordinary, that no accounts have been yet laid before you, of our success here from the same experiments. That no want of attention, therefore, may be attributed to those here, who have been hitherto conversant in these inquiries, I thought proper to apprise you, that, though several members of the Royal Society, as well as myself, did, upon the first advices from France, prepare and set up the necessary apparatus for this purpose, we were defeated in our expectations, from the uncommon coolness and dampness

dampness of the air here, during the whole summer. We had only at London one thunder-storm; viz. on July 20; and then the thunder was accompanied with rain; so that, by wetting the apparatus, the electricity was dissipated too soon to be perceived upon touching those parts of the apparatus, which served to conduct it. This, I say, in general prevented our verifying Mr. Franklin's hypothesis: but our worthy brother Mr. Canton was more fortunate. I take the liberty, therefore, of laying before you an extract of a letter, which I received from that gentleman, dated from Spital-square, July 21, 1752.

“ I had yesterday, about five in the afternoon, an  
 “ opportunity of trying Mr. Franklin's experiment  
 “ of extracting the electrical fire from the clouds;  
 “ and succeeded, by means of a tin tube, between  
 “ three and four feet in length, fixed to the top of  
 “ a glass one, of about eighteen inches. To the up-  
 “ per end of the tin tube, which was not so high  
 “ as a stack of chimnies on the same house, I fastened  
 “ three needles with some wire; and to the lower  
 “ end was solder'd a tin cover to keep the rain from  
 “ the glass tube, which was set upright in a block  
 “ of wood. I attended this apparatus as soon after  
 “ the thunder began as possible, but did not find it  
 “ in the least electrified, till between the third and  
 “ fourth clap; when applying my knuckle to the  
 “ edge of the cover, I felt and heard an electrical  
 “ spark; and approaching it a second time, I re-  
 “ ceived the spark at the distance of about half an  
 “ inch, and saw it distinctly. This I repeated four  
 “ or five times in the space of a minute; but the  
 “ sparks

“ sparks grew weaker and weaker ; and in less than  
 “ two minutes the tin tube did not appear to be  
 “ electrified at all. The rain continued during the  
 “ thunder, but was considerably abated at the time  
 “ of making the experiment.” Thus far Mr. Canton.

Mr. Wilson likewise of the Society, to whom we are much obliged for the trouble he has taken in these pursuits, had an opportunity of verifying Mr. Franklin's hypothesis. He informed me, by a letter from near Chelmsford in Essex, dated Aug. 12, 1752. that, on that day about noon, he perceived several electrical snaps, during, or rather at the end of, a thunder-storm, from no other apparatus than an iron curtain-rod, one end of which he put into the neck of a glass phial, and held this phial in his hand. To the other end of the iron he fasten'd three needles with some silk. This phial, supporting the rod, he held in one hand, and drew snaps from the rod with a finger of his other. This experiment was not made upon any eminence, but in the garden of a gentleman, at whose house he then was.

Dr. Bevis observed, at Mr. Cave's at St. John's gate, nearly the same phenomena as Mr. Canton, of which an account has been already laid before the public.

Trifling as the effects here mention'd are, when compared with those, which we have received from Paris and Berlin, they are the only ones, that the last summer here has produced ; and as they were made by persons worthy of credit, they tend to establish the authenticity of those transmitted from our correspondents.

I flatter myself, that this short account of these matters will not be disagreeable to you; and am, with the most profound respect,

Gentlemen,

Your most obedient humble servant,

Lincoln's-Inn-Fields,  
Dec. 20, 1732.

W. Watfon.

XCVI. *Extract of a Letter from Mr. Brown, Apothecary, at Salisbury, to Mr. Wm. Watfon, F. R. S. concerning the Success of Inoculation there.*

Read Dec. 21, 1752,  
and here printed with  
Additions:

I AM much obliged to you for the observations, which you were so kind as to send me, concerning the method of inoculating for the small-pox, and the subsequent treatment of that distemper. This I should not have deferred till now, but that I was desirous of sending you some account of our success therein.

Since the receipt of your letter, inoculating has been very much practised here, and with great success; of which the account I now send may be looked upon as pretty authentic. From the 13 of August, to the beginning of February, have been inoculated, in this city and neighbourhood, four hundred and twenty-two persons. On five or six of these, to my knowledge, it had no effect; though on one the experiment was tried a second time.

Of

Of this whole number four have died; one of which was a patient of mine, who, I am inclined to think, did not do justice to this method: but that is submitted to better judgment; for the day, on which the operation was performed, the patient's blood had been heated violently by exercise, and suddenly chill'd again, by putting on clean linen, just before the operation was performed; which, I apprehend, is receiving the infection in an inflamed state of blood: but with this I was not the least acquainted, till about six hours before the patient's death.

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A N  
I N D E X  
T O T H E  
Forty-seventh VOLUME  
O F T H E  
*Philosophical Transactions,*

For the YEARS 1751 and 1752.

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## F I N I S.

### E R R A T A.

Page 43, Note l. 3, for *June* 4, read *May* 28. Page 64, l. 4, for *Tab. Fig.* read *Tab. I. Fig.* P. 421, l. 34. *for* will have power, read will have no power. P. 422, l. 2. after 1000 times, *add*, and frequently 2000. *Ibid.* l. 5. *dele* same. *Ibid.* l. 14. *for* been at it, read been done at it. *Ibid.* l. 16. *for* vapours read moisture. P. 424. l. 15. *for* with several joints, read, and several joints of the pump. *Ibid.* l. 25. after *cock* make a comma, and blot out that after *valve* in the next line. P. 415, l. 4. *for* use read *rile*. P. 496, l. 10. *dele* new method. *Ibid.* l. 28. *for* figure annexed read Fig. 6. Plate xviii.









